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AND
JOURNAL OF SCIENCE.

CONDUCTED BY

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"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." *Jusr. Lips. Monit. Polit. lib. i. cap. 1.*

VOL. XVII.

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PLATES.

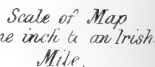
- I. Illustrative of Mr. GRIFFITH's Paper on the Mineral Structure of
the South of Ireland.
- II. Illustrative of Dr. FARADAY's Letter to M. Gay-Lussac, on Magneto-
electric Induction*.

* This Plate will be given in one of the Number of the next Volume.

ERRATA, &c.

P. 145, line 17 from the bottom : the following reference ought to have been *inserted* in this place : " An abstract of Prof. Faraday's Seventeenth Series of Researches in Electricity appeared in vol. xvi. p. 336."

P. 370, line 12 ; p. 372 line 15 ; and p. 374, line 8 from the bottom, *for* H. G. ARMSTRONG, *read* W. G. ARMSTRONG.



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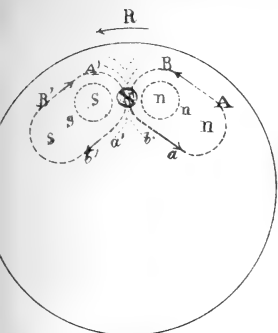


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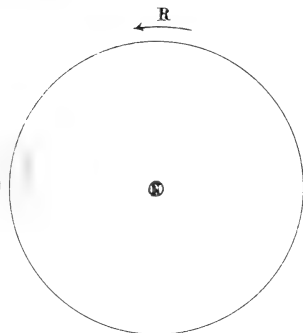


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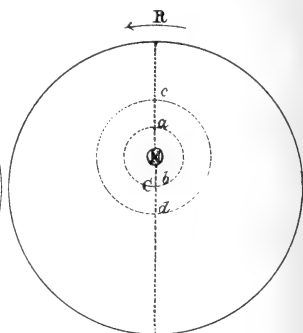


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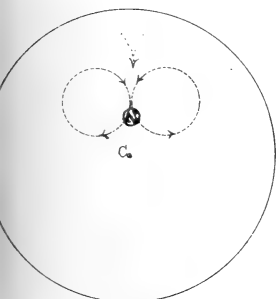


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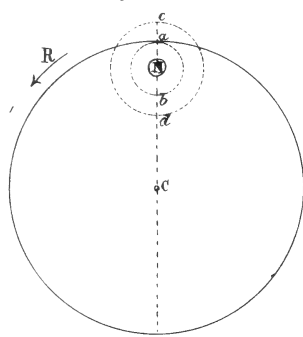


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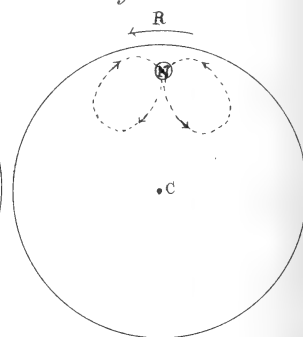


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Fig. 7.

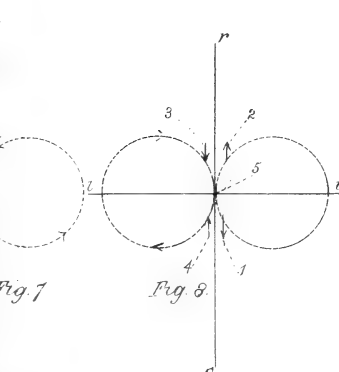


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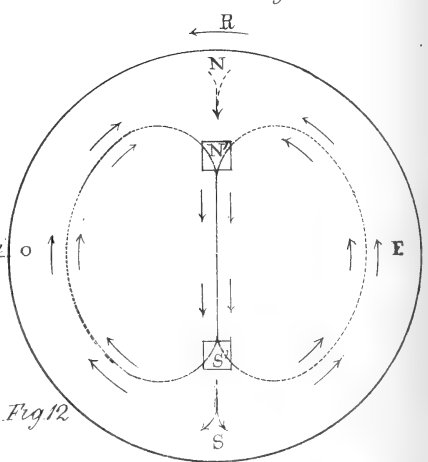


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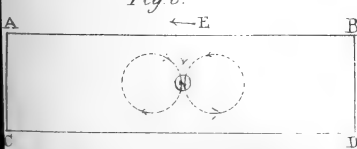


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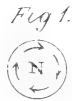


Fig. 1.

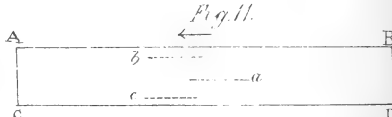
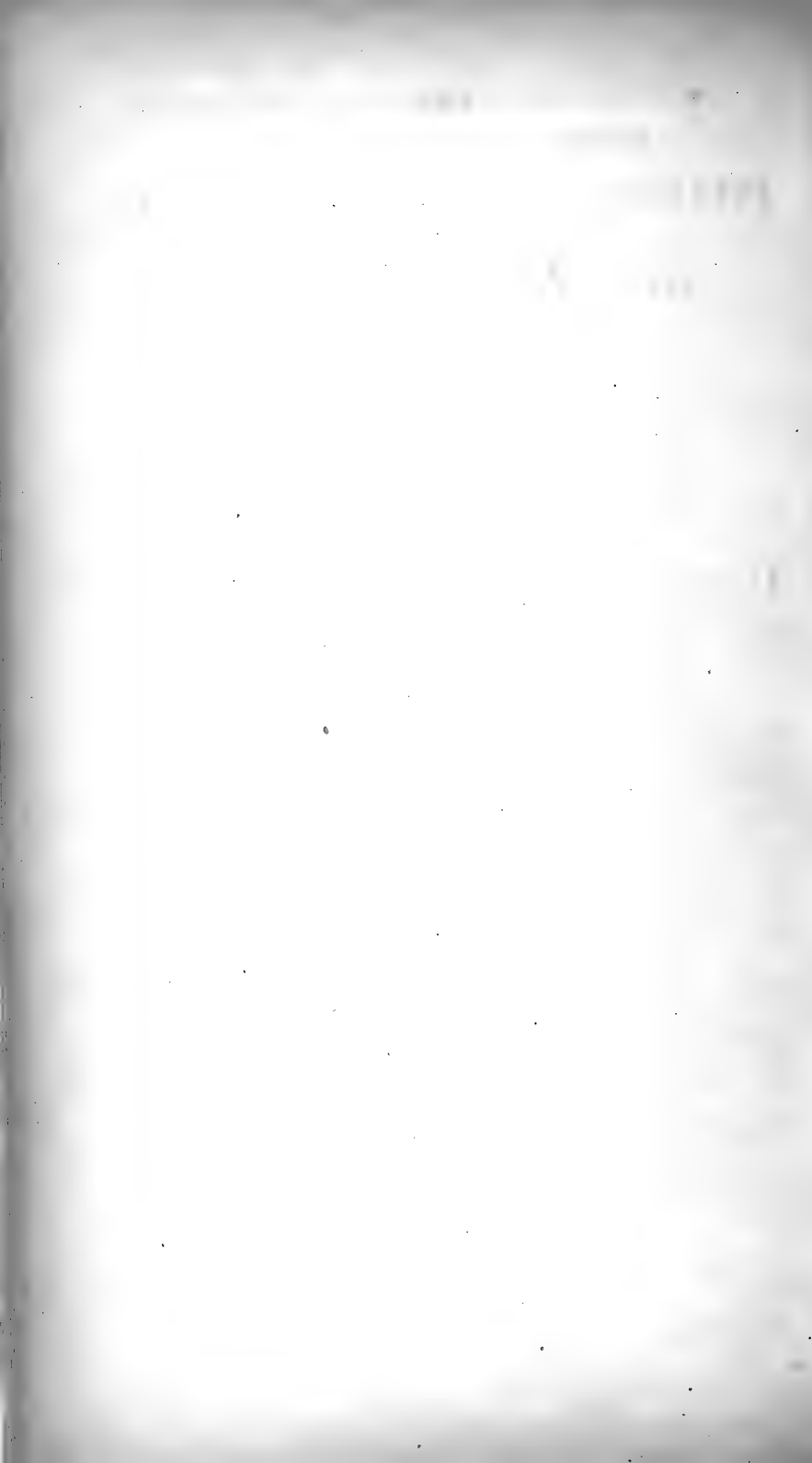


Fig. 11.



THE
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[THIRD SERIES.]

JULY 1840.

I. *On the Iodide of a new Carbo-hydrogen.* By JAMES F. W. JOHNSTON, F.R.S.*

IF coal gas be made to pass slowly and for a length of time over pure iodine, the latter substance moistens, and is partly changed into a dark-brown liquid, which effervesces with alkaline carbonates, showing the presence of hydriodic acid. After some hours, colourless prismatic crystals shoot out from the iodine, and clothe the interior of the vessel, and ultimately the whole is changed into a mixture of different compounds forming an olive-coloured substance, partly coating thickly the sides of the vessel, and partly constituting an unctuous mass, with the dark fluid at the bottom.

The liquid contains free iodine and hydriodic acid. When washed out from the solid portion by alcohol and neutralized by caustic potash, the solution gives a yellow precipitate, consisting of a mixture of Faraday's iodide of carbo-hydrogen ($\text{H}_2 \text{C}_2 \text{I}$)† and of iodide of formyle (iodoform $\text{H C}_2 \text{I}_3$).

The solid product being exposed to the air loses its unctuousity. If broken up and examined by the microscope it is seen to consist of a congeries of colourless prisms ($\text{H}_2 \text{C}_2 \text{I}$) mixed with another substance, which is amorphous, and of a dark green almost black colour. Alcohol separates the former, or if the mixture be exposed to the air they volatilize, leaving the dark green substance nearly pure.

The production of the hydriodic acid and of the iodides of formyle and of carbo-hydrogen is easily understood. Coal gas contains probably more than two equiatomic compounds of carbon and hydrogen; at least two, C H_2 and $\text{C}_2 \text{H}_2$, the

* Communicated by the Author.

[† Mr. Faraday's account of this substance will be found in *Phil. Mag.* First Series, vol. lix. p. 352. EDIT.]

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light carburetted and olefiant gases, are present. The latter would furnish the three compounds obtained in this experiment, as shown by the following formula :



that is to say, one atom of olefiant gas decomposes to form hydriodic acid and formyle, while another unites with iodine* directly. Still this does not represent the action *quantitatively*, since the proportion of the $\text{C}_2 \text{H}_2 \text{I}$ is much greater in actual experiment, and appears also to be variable.

Such is the action in close vessels provided only with a small aperture to allow the current of gas to pass out very slowly; but since these three compounds are all volatile, it is easy to understand how only the solid dark green fixed substance should be obtained when the iodine is placed in an open vessel and a current of coal gas is made to stream upon it. In this way it was first obtained by Mr. Kemp of Edinburgh, to whom the discovery of this substance is due, and who several years ago presented me with a specimen prepared by exposing iodine for several days to the action of an open jet of coal gas. I am not aware how far Mr. Kemp has since studied the action in close vessels.

I. This substance is of a dark olive green colour, is without taste, emits a slight odour of naphtha, is brittle, and has a density of about 0.95. It is insoluble in water, and in boiling alcohol or æther. Treated with hot nitric acid it becomes yellow and dissolves. With muriatic acid either in the form of gas or of liquid acid, it undergoes no change. Sulphuric acid aided by heat decomposes it. It blackens and gives off iodine vapour and sulphurous acid, leaving undissolved a very bulky charcoal. Dry chlorine slowly changes its colour to a dark brown. If previously moistened with alcohol it becomes

* During the combination of chlorine with olefiant gas a portion of muriatic acid is formed, a fact inconsistent with the idea of a *direct* union of the two substances to form $\text{H}_2 \text{C}_2 \text{Cl}$: may not an equivalent proportion of the volatile chloroform be produced, as in the above formula, substituting Cl for I? Felix d'Arcet (*Ann. de Chim. et de Phys.*, lxvi. p. 108.) has stated, that during this action of chlorine on olefiant gas, a second oily liquid is formed, represented by $\text{C}_4 \text{H}_4 \text{Cl O}$, to which he gives the name of *chloretheral*, but which Berzelius with great probability I think, considers to be a compound of the chloride with the oxide of *elayle* ($\text{C}_2 \text{H}_2 \text{Cl} + \text{C}_2 \text{H}_2 \text{O}$). This explanation of the production of muriatic acid, however, implies that the gases employed are always more or less moist. Regnault accounts for the presence of the acid by representing the oily compound by the rational formula ($\text{C}_2 \text{H}_3 \text{Cl} + \text{H Cl}$) part of which is decomposed during the process, and H Cl evolved. But Löwig and Wiedman have shown that $\text{C}_2 \text{H}_3$ (acetylene) does not preexist in the oil, though it may possibly be formed by its decomposition. See Poggendorff's *Annalen*, xlix. p. 133.

yellow by the action of chlorine. In hot solutions of carbonated alkalies it is partly decomposed, iodine being separated, but its colour remains unchanged. Heated to 212° Fahr., it slowly but sensibly loses weight, evolving the odour of naphtha and a little iodine. At a higher temperature it gives off a volatile combustible liquid resembling naphtha, which burns with much smoke, and if the heat be still increased, iodine vapour appears in large quantity colouring the naphtha (?) dark brown, and a bulky shining charcoal remains behind. It is not unlikely that hydriodic acid may also be among the products.

Burned with oxide of copper this substance gave the following results :

1. 8.77 grs. gave $\ddot{C} = 18.56$ and $\dot{H} = 4.94$ grs.
2. 8.77 grs. gave $\ddot{C} = 18.15$ and $\dot{H} = 4.855$ grs.
3. 6.137 grs. gave $\ddot{C} = 12.316$ and $\dot{H} = 3.505$ grs.

These are equivalent, per cent., to

	1.	2.	3.
Carbon....	= 58.203	57.225	55.490
Hydrogen =	6.258	6.151	6.346
Iodine.....	= 35.539	36.624	38.164
	<hr/> 100	<hr/> 100	<hr/> 100

It was not till I observed the discrepancy between the first and second analyses that I studied the action of a temperature of 212° Fahr. on this compound, and found that it was slowly decomposed, and iodine expelled from it by this degree of heat. The third analysis therefore was made with more precaution, and care was taken to avoid decomposition by the application of heat while pumping out the moisture from the oxide of copper. In this analysis therefore the chances of error were least, and the result agrees very closely with the formula $C_{30} H_{20} I_1$ since

	Calculated.	Experiment.
30 carbon ... =	2293.110	55.490
20 hydrogen =	249.592	6.346
1 iodine.... =	1578.290	38.164
	<hr/> 4120.992	<hr/> 100

The excess of hydrogen is due to the more imperfect manner in which it was necessary to pump out the water in order to avoid the separation of iodine, as had probably been the

4 Prof. Johnston on the Iodide of a new Carbo-hydrogen.

case in the previous analysis*. The experimental result however may be reconciled to the formula ($C_{30}H_{20} + HI$) which gives 6.340 of hydrogen per cent., a quantity so near to that found as to leave nothing for the ordinary error of analysis. The action of chlorine hereafter described, if the results are to be depended upon, gives a probability to this formula, in addition to that which is derived from the rational formula, adopted by Liebig to represent the constitution of what by other chemists are still regarded as chlorides and iodides of olefiant gas (elayle) and some other carbo-hydrogens.

It has been already stated, that when boiled in carbonated alkalies the colour of this compound remains unchanged, and that it undergoes decomposition. The decomposition however is only partial. 4.51 grs. boiled in a concentrated solution of carbonate of soda, and afterwards washed and dried at a gentle heat, still weighed 3.51 grs. having lost 22.17 per cent. The whole of the iodine therefore is not separated by this process; it may however be completely separated by mixture with pure carbonate of soda, and gradually heating over the lamp to a temperature below redness.

In this way I obtained by means of nitrate of silver an approximation to the quantity of iodine, which however was too rude to be worthy of insertion in the present paper. I was, at the time of making the experiment, unacquainted with the more perfect method of estimating the quantity of iodine since published by Lassaigne.

II. Diffused through water and subjected to the action of chlorine, the green colour of this compound is slowly changed to brown, but the action is much more rapid and complete when the iodide is reduced to fine powder, diffused through alcohol, and submitted to a current of chlorine. Thus treated it speedily acquires a bright yellow colour, combining with chlorine and yielding the iodine to the supernatant liquid, in which it is readily recognised.

I subjected to analysis a portion of the substance thus prepared, after washing with alcohol and drying at 212° Fahr. When heated in a close tube it gave off no iodine.

a. 8.45 grs. gave $\bar{C} = 18.81$ grs. and $\bar{H} = 4.545$ grs.

* 2.795 grs. heated to 212° for some time, lost 0.295 gr., and at a subsequent weighing the loss amounted to 0.485 gr. Still it seems possible to preserve it for a long time at ordinary temperatures and in close vessels without sensible decomposition. One of the specimens employed in the above analysis was prepared by Mr. Kemp, and had been in my possession several years.

The water in the chloride of calcium tube reddened litmus, indicating the presence of muriatic acid, by which the weight of water would be in some measure increased.

b. 7.61 grs. heated with dry carbonate of soda, gave 7.033 grs. of chloride of silver or 22.8 per cent. of chlorine.

c. 4.462 grs. heated in like manner, but with more care, gave 4.517 grs. of chloride of silver or 24.12 per cent. of chlorine.

These results give for the composition of the yellow matter

	A	B	
Carbon...	= 61.55		= 30 atoms.
Hydrogen	5.98		17.8 —
Chlorine...	24.12	22.8	2.02 —
Oxygen ...	8.35		3.11 —

100

This result indicates the irrational formula $C_{30} H_{17} Cl_2 O_3$, which gives

30 carbon =	2293.11	=	62.13 per cent.
20 hydrogen	212.15		5.75 —
2 chlorine	885.30		23.99 —
3 oxygen	300.00		8.13 —
	3690.56		100.

The elements contained in the above irrational formula are capable of being arranged in several rational positions.

The green iodide being $C_{30} H_{20} + I$,

The yellow substance may be $(C_{30} \frac{H_{17}}{O_3} + Cl) + Cl$ (1), in which three of hydrogen are replaced by three of oxygen, the atom of iodine by one of chlorine, and the whole combined with another atom of chlorine—the oxygen being derived from the alcohol, which was undergoing a simultaneous decomposition by the action of chlorine.

Or, it may be $C_{30} \frac{H_{16}}{Cl} O_3 + H Cl \dots$ (2.)

Or — $C_{30} \left\{ \begin{array}{l} Cl_2 \\ H_{16} \\ O_2 \end{array} \right. + H O \dots$ (3.)

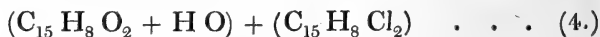
in both of which the principle of substitution is equally evident. Were we to represent the green compound by $C_{30} H_{20} + H I$, there would even be a conservation of the original *type* of composition*. This type may also be supposed to be sufficiently preserved in the radical, and that it is owing

[* See the memoir of M. Dumas, p. 442 of the last volume.—EDIT.]

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to the presence of much muriatic acid in the solution, derived from the action of the chlorine on the alcohol, that this radical $\frac{30}{20}$ R unites with H Cl (2°) instead of simply with Cl, when it might have been represented by a formula* the exact counterpart of that which indicates the substance from which it is derived.

There is, however, still another mode of representing the rational constitution of this substance, which while it is accordant with the *facts* on which Dumas's views are founded, is inconsistent with the *principle* of the conservation of types. The yellow compound may be



consisting of equal atoms of an analogous oxide and chloride of the radical $C_{15} H_8$ or $C_{30} H_{16}$, with the former of which is combined also an atom of water. This mode of representing it is accordant with the views of Berzelius, and is supported by many interesting and striking analogies.

We ought indeed to distinguish carefully between the *fact* of the mutual substitution of hydrogen and chlorine, and the *theory* of the persistence of types, or the opinion that the element which replaces performs the same function in the organic compound as that which is replaced. Of the former there can be no doubt, while the adoption of the latter as a principle is as yet attended with many difficulties and apparent anomalies, which do not present themselves when we regard these altered compounds after the manner in which our yellow substance is represented in formula 4.

New views all tend to hasten forward science, but a new view is not in itself necessarily an advance. It may often serve as a useful guide-post, when it does not directly help us on our way. Such good results are sure to follow from the discussion of the theory of substitutions, though all the views of its eminent author should not find a permanent place in the science.

I speak with the less confidence in regard to the above formulæ, because I am sensible that the examination of the two compounds described in this paper is by no means complete: a more careful research into the properties and chemical relations, especially of the first of them, would be likely I think to lead to interesting results.

The analyses above given were made as far back as 1838, and in February 1839, and the investigation was left unfinished till I should obtain a fresh supply of the compound. My

* $C_{30} \frac{H_{16}}{Cl} O_3 + Cl.$

attention has recently been recalled to the subject by a paper on *Hellenine*, by M. Gerhardt (*Ann. de Chim. et de Phys.* vol. lxxii. p. 163), in which he gives for this substance the formula $C_{15}H_{10}O_2$ or $C_{30}H_{20}O_4$, containing apparently a radical isomeric with the carbo-hydrogen $C_{30}H_{20}$, which exists in the iodide above described. By the action of chlorine, hellenine becomes $(C_{15}H_{10}O_4 + C_{15}H_{10}Cl_4)$ according to Berzelius, or $(C_{15}H_9ClO_2 + HCl)$ according to Dumas, in which we see a considerable analogy with the formulæ for the oxichloride above described. By the action of anhydrous phosphoric acid on hellenine, a yellow liquid carbo-hydrogen is produced, to which M. Gerhardt gives the name of hellenene, and which is represented by the formula $C_{15}H_8$, — the hypothetic radical which enters into the constitution of our oxichloride as it is represented in the formula (4).

These interesting approximations indicate a series of comparative experiments, to which the iodide described in the present paper might be subjected, with the hope of throwing new light on the nature of the ever-varying isomeric modifications, of which the compounds of carbon and hydrogen are susceptible. I hope to be able soon to return to the subject with a view to this investigation.

In regard to the presence of a carbo-hydrogen represented by the formula $C_{30}H_{20}$ in coal gas, it need not excite our surprise if many other such compounds should hereafter be met with among the volatile and gaseous products obtained from the distillation of coal. When we consider how many less volatile substances of this class have been extracted by Pelletier and Walter* from the products of the distillation of resin for the manufacture of gas, and how many more volatile ones have been separated by Couerbe† from the gas thus produced when subjected to pressure, we shall be prepared to expect in coal gas also, the vapours of many other volatile substances in addition to those which have hitherto been detected.

I have not as yet proposed any name for the supposed radical $C_{30}H_{20}$. It belongs to the same group as mesitylene (Enyl of Berzelius) = C_6H_4 and retinyle $C_{18}H_{12}$, in both of which the elements are in the ratio of 3 to 2. It would be exceedingly desirable to adopt the system of nomenclature proposed by Berzelius for these compounds, in which the name is compounded of the Greek numerals expressive of the number of atoms of each element which are contained in the compound. But that such names may be universally adopted, it is necessary that the same atomic weights should also be

* Poggendorff's *Annalen*, vol. xlv. p. 81.

† *Ann. de Chim. et de Phys.*, vol. lxix. p. 148.

generally received. In the present case, for example, our green compound would be represented

By $C_{30}H_{40} + I_2$ according to Berzelius.

By $C_{60}H_{40} + I_2$ according to Dumas.

And by $C_{30}H_{20} + I$ according to British chemists. The compounds of carbon and hydrogen therefore, on the principle of Berzelius, which abstractedly is very valuable as a guide for general nomenclature, would receive in the works of different chemists at least two, and sometimes three different names of foreign origin. Trivial names therefore derived as heretofore from different and various sources, will be likely in the present state of the science to create much less confusion in our rapidly extending, already exceedingly difficult and almost Protean nomenclature.

Durham, May 16, 1840.

II. *On the Optical Characters of Greenockite (Sulphuret of Cadmium).* By JAMES D. FORBES, Esq., F.R.SS. L. and Ed., Professor of Natural Philosophy in the University of Edinburgh.*

IT appears from a late number of Professor Jameson's Journal, that the crystallographic characters of this new mineral, as examined by Mr. Brooke, remain ambiguous, and that the crystals sent to him for examination do not enable him positively to say whether it belongs to the rhombohedral or to the prismatic system; it may therefore be interesting to state that I have discovered that it possesses only *one* axis of double refraction in the direction of the axis of the pyramid or prism in which it usually crystallizes, and consequently there can be no doubt that greenockite is a rhombohedral cadmium blende.

Edinburgh, May 18, 1840.

III. *On a new Species of Biliary Calculus.* By THOMAS TAYLOR, M.R.C.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WHILST engaged in examining the extensive collection of calculi in the museum of the Royal College of Surgeons, which had been entrusted to me for that purpose by the Board of Curators, I remarked, among those in the Hunterian collection, one, which from its extreme lightness and peculiar

* Communicated by the Author. We were favoured with this article in the middle of May, but from an oversight which we regret its insertion was omitted.

appearance, I was led to suspect had been incorrectly described in the manuscript catalogue as consisting of the mixed phosphates.

This calculus was externally of a dirty white colour, and had the greasy feel of cholesterine calculi; it floated on water, and when applied to the tongue, left an impression of bitterness. It was of an oval figure slightly flattened, one inch and a half in length, rather better than an inch in thickness, and about one inch and a quarter in breadth, but being broken in this direction its exact measurement could not be ascertained. It readily yielded to the knife, and the cut surface presented a polished appearance: its structure was lamellar, being composed of white and reddish-yellow layers arranged concentrically and alternating with each other. The layers were easily separable: at its centre there was a small vacuity.

When heated before the blowpipe it readily fused, then caught fire, burning with a clear flame and giving out the smell of animal matter, but nothing of a urinous character. It left a carbonaceous residue, which by raising the heat was converted into a white ash. This ash was alkaline, dissolved in water and dilute acetic acid, and the solutions gave a white precipitate with oxalate of ammonia; it was therefore lime.

When digested in boiling water, the water became slightly brown, but no apparent solution took place: the water on evaporation left a transparent yellowish-brown residue, which had a bitter taste and resembled inspissated bile.

Boiling alcohol extracted from it only a minute quantity of white fatty matter, which was deposited on cooling.

A solution of caustic potass removed the whole of the colouring matter, but the rest of the calculus was unacted on: the potass solution was dirty green, and when neutralized with muriatic acid deposited a scanty precipitate of the same tint.

When digested in nitric acid, effervescence took place, with the escape of a little nitrous acid; it then melted into a transparent oil, which on cooling concreted into a white fatty matter. This substance, when washed with distilled water, melted at a temperature much below that of boiling water.

When, instead of nitric acid, muriatic or acetic acid was employed, the portion of calculus did not melt until it had been removed from the acid; it then presented similar appearances to that obtained by the action of nitric acid; consequently this white fatty matter was not formed by the action of the nitric acid.

In all these cases the acids retained lime in solution. The fatty matter separated by the action of acids was partially soluble in boiling alcohol, and the solution on cooling deposited shining crystalline scales. With caustic potass it formed a ropy almost gelatinous solution, and was precipitated in white flakes on the addition of an acid. A small piece being placed upon the ball of a thermometer previously heated, began to solidify when the temperature had sunk to about 135° Fahr.

From these experiments I concluded that this calculus consisted of margarate, or stearate of lime, mixed probably with the oleate of the same base and some of the other constituents of the bile. That the lime was in combination with the fatty acid, was indicated by the insolubility of the calculus either in alcohol or caustic alkaline solutions, until it had been previously digested in some acid.

The minute quantities on which I had hitherto operated prevented me from determining whether only one or more of the fatty acids were present. The following analysis was therefore made.

Analysis.—12·80 grs. of the calculus previously dried *in vacuo* over sulphuric acid were boiled in distilled water: a peculiar odour was given off, and the water acquired a yellowish-brown colour: being evaporated to dryness it left a transparent resinous-looking residue, which weighed 0·84. This residue when digested in alcohol left 0·24 in the form of dirty yellow flakes, which in distilled water swelled up and ultimately dissolved, forming a solution which in its chemical characters exactly resembled that of the mucus of the gall bladder.

The alcoholic solution being evaporated to dryness, the residue was redissolved in water; the solution was intensely bitter; with muriatic acid it gave a copious viscid precipitate: acetate of lead produced likewise a viscid precipitate, and the supernatant liquor when clear was again troubled by a solution of subacetate of lead.

The 0·84 consisted therefore of mucus of the gall bladder 0·24; inspissated bile 0·60.

After water had extracted from the calculus all that it was capable of dissolving, it was treated with successive portions of boiling alcohol sp. gr. ·830.

The first alcoholic solution on cooling deposited a white matter, which did not readily redissolve in hot alcohol or æther, but was acted upon by acetic acid. It appeared to be part of the calculus that had been dissolved unchanged; the quantity was however too minute to be estimated. The alcoholic solutions were filtered, and being mixed together, the

whole was gently evaporated; as the liquid became concentrated, it deposited some white fatty matter and acquired a yellow tinge; a residuum was ultimately left, which had the appearance of a mixture of a fluid and concrete oleaginous substance. On the application of heat it became a yellow oil, which on cooling only partially solidified: it weighed 0.47.

It strongly reddened litmus paper; dissolved readily in a cold solution of caustic potass; and was precipitated in soft flakes on the addition of an acid.

This substance consisted therefore of oleic acid, mixed with margaric or stearic acid.

Strong acetic acid diluted with twice its bulk of water was now poured over the calculus, and the action of the acid aided by a gentle heat. The insoluble residue was collected on double filters, washed, and dried.

The acetic solution with its washings was reduced to a small bulk, and a solution of ammonia added; after the lapse of several hours no precipitate appearing, the excess of ammonia was nearly neutralized by a solution of oxalic acid: a white precipitate fell, which when washed, dried, and heated to dull redness in a platina crucible, left 2.09 carbonate of lime = 1.17 lime.

The remaining liquid being evaporated to dryness and the ammoniacal salts expelled, a residuum was left which weighed 0.10: water dissolved a portion of this; the solution was alkaline, and when evaporated minute crystals were formed, which slightly effervesced in acetic acid: their solution not precipitating chloride of platina, leaves little doubt of their being carbonate of soda: the small portion which remained undissolved proved to be carbonate of lime.

The matter left upon the filter after the action of the acetic acid was again digested in boiling alcohol, a considerable portion dissolved, and the remainder had acquired a much deeper colour: it was collected on the same filters, which were repeatedly washed with boiling spirit; when dried and weighed against the outer filter it amounted to 0.86.

This substance possessed a brownish-yellow colour. It dissolved in solutions of caustic and carbonate of potass, forming solutions having nearly the same colour.

Muriatic acid rendered it green, and when added to its alkaline solution threw down green flocks.

With nitric acid it formed a red solution.

This substance was therefore identical with the colouring matter of the bile, and which forms the principal constituent of the biliary calculi of oxen.

The alcoholic solutions were concentrated by careful di-

stillation in a small retort: the liquid remaining in the retort, when cold, formed a soft crystalline mass, composed of brilliant plates and having a pearly lustre, very much resembling margaric acid.

This substance, when fused and kept for some time *in vacuo* over sulphuric acid, weighed 8.88. It melted at 136° Fahr., and on cooling became a crystalline solid, reddened litmus paper, and was easily soluble in a cold solution of caustic potass; the solution when concentrated was ropy and gelatinous; when dilute it formed a slightly milky mixture with minute glistening particles floating in it; on the addition of an acid, the substance was thrown down in the form of white flakes, which possessed the same properties as before solution. When boiled with the alkaline carbonates, it was dissolved, with the escape of carbonic acid. By redissolving it in hot alcohol, crystalline plates were deposited on cooling, which after washing with cold spirit fused at about 140° . The low fusing point of this substance evidently indicates the presence of oleic acid. In order to ascertain whether the crystals fusible at 140° were pure margaric acid or stearic acid rendered more fusible by an admixture of oleic acid, they were again dissolved in warm spirit, and the crystals as soon as formed dried by compression between folds of blotting paper; by repeating this process two or three times, the fusing point was raised to nearly 160° . This must therefore be regarded as pure stearic acid; and as I find that both stearic and margaric acids require to be several times recrystallized from their alcoholic solutions to free them from even small quantities of oleic acid, and as no decided indication of the presence of margaric acid could be detected in the mother liquors, I am inclined to believe that oleic acid had only been separated by the above treatment, and that consequently margaric acid did not enter into the composition of the calculus. It would however be impossible to speak decidedly on this point.

The result of the analysis is as follows:

Stearic acid mixed with a small pro- portion of oleic acid.....	} 9.35
Lime	1.17
Soda with a trace of lime.....	0.05
Yellow colouring matter of the bile.....	0.86
Inspissated bile	0.60
Mucus of the gall-bladder	0.24
	<hr/> 12.27
Loss	53
	<hr/> 12.80

The greater part of the loss in this analysis should be added to the stearic acid, as owing to the sudden extrication of vapour while under the receiver of the air-pump a small part of the acid was thrown out; but I prefer giving the quantities actually obtained to making any allowance for known sources of error.

The composition of the calculus clearly points out its biliary origin, but whether taken from man or the brute must remain doubtful, as there is no history to guide us.

Stearic, margaric, and oleic acids exist in the bile of oxen in combination with soda; and according to Lecanu and Casa-seca*, stearic and oleic acid in that of man; the latter I have frequently detected, but I cannot find that these acids have ever been noticed as entering into the composition of biliary concretions, much less forming the prominent constituent. I was unable to detect the slightest trace of these acids in five specimens selected from about 200.

From cholesterine calculi it is readily distinguished by the absence of any crystalline structure when broken, which unless the quantity of colouring matter be very large is always more or less apparent in that variety; also by its insolubility in alcohol or æther and by readily dissolving in these menstrua, and in a cold solution of caustic potass after it has been acted upon by an acid.

Before I conclude, I am anxious to rectify an error which I inadvertently committed in my paper on the calculi in the museum of St. Bartholomew's Hospital†. I there stated that urate of ammonia had always been confounded with the uric acid variety in the tables that had been published on the relative frequency of the different species of calculus‡. This was not, however, the case in a paper by Dr. Yelloly, in the Philosophical Transactions for 1829-30, containing the analysis of the Norwich collection, and I regret that I have not had an opportunity of making this acknowledgement sooner.

April 8th, 1840,
New Bridge Street, London.

THOMAS TAYLOR,
M.R.C.S.

N.B. A notice of this calculus was sent to the Board of Curators in March 1839, but a variety of circumstances have delayed its publication.

* Gmelin's *Handbuch der Chemie: Journal de Pharmacie*, 12.

† L. & E. Phil. Mag. vol. xii. p. 412.

‡ *Ib.* p. 414.

IV. *On the Anatomy of the Brain.* By Dr. FOVILLE, M.D. Par.*

IT is more than twelve years since Dr. Foville laid before the Royal Academy of Sciences of Paris some highly interesting discoveries respecting the anatomical structure of the brain. They were the results of laborious researches in what at that period he had already been long engaged. At the time they attracted the careful attention of the anatomical members of the Institute, and more especially of the late Baron Cuvier, and of Professor Blainville, and an able and highly favourable report on the subject was presented to the Academy by the latter *savant*. Translations of this Report and of Dr. Foville's Memoir were published soon after in the *Annals of Philosophy*†. From that time to the present the Doctor has been pursuing his investigations, and has made new discoveries as well as confirmed his old ones, and has been engaged in preparing a detailed and extended work on the anatomy, physiology, and pathology of the brain and spinal cord. He laid a condensed view of the anatomical facts before the Medical Section of the British Association at its meeting in Birmingham, and has presented a further memoir to the Academy of Sciences of Paris. The Doctor has thrown the substance of both papers into the following description.

Medical men in general are so well aware of the importance, and at the same time of the difficulty of the study of the nervous system, that the physician who attempts to communicate to his professional brethren the result of his researches, may flatter himself that he is speaking on a subject which cannot fail to be interesting. Hence in desiring to communicate some results of my anatomical researches concerning the brain, I need not solicit the indulgence of the meeting for the subject itself, which has for many years engaged my attention, but I must apologise for the imperfect style of the present sketch. I arrived in London only a few days ago in order to visit my friend Dr. Hodgkin, and had not anticipated his inviting me to join him at the meeting of the British Association for the Advancement of Science, and present to this meeting an abstract of my researches into the structure of the encephalon. Having left my manuscript, now nearly ready for publication, in France, and not having time and opportunity to procure new preparations from which I might write the present essay, I have been obliged to depend on

* Communicated by Dr. Hodgkin.

† *Phil. Mag. and Annals*, N.S. vol. v. p. 278.—EDIT.

my memory for the description of some structural arrangements which have arrested my attention.

When we look at the works which treat of the anatomy of the brain, we find that they may be distinguished into two classes. Some being designed to illustrate the form of the brain in general, and to indicate the particulars of its external surface as well as of its ventricular cavities, only exhibit its substance by sections, which destroy the arrangement of the parts without showing their structure. Others, being composed by authors aware of the imperfection of those processes by which nothing but superficial appearances and casual sections are exhibited, aim at elucidating the mysteries of the cerebral organization by studying the composition and arrangement of the substances constituting the encephalic masses.

It is needless in speaking to so enlightened an assembly as this to draw any comparison between the advantages of these two different methods. Centuries have passed since the superiority of that which is designed to ascertain the intimate composition and structure of the parts was first recognised by some anatomists. The celebrated Willis has insisted as strongly as any modern writer on the advantages of the process alluded to; and Malpighi proved its excellence when he so accurately described the granular disposition of the gray matter and the fibrous structure of the white. Nevertheless in spite of these superior men, their improved views did not generally prevail in the schools; and when at the end of the last century, Reil, and at the beginning of the present, Gall, undertook to prove the fibrous structure of the white substance of the brain, they thought they were announcing an anatomical discovery.

Reil by his writings, and Gall by his lectures and publications in which he was associated with Spurzheim, have propagated their ardour for sound anatomical inquiries. But notwithstanding their efforts, the practice of cutting through the brain is not yet abandoned, though it is generally understood that this old and imperfect method is insufficient of itself, and that it is by combining with it more appropriate means of investigation that facts which twenty years ago were controverted are now incontestably demonstrated. Thus, for example, all anatomists of the present day concur in the view that the white substance is fibrous, and the gray granular; and at the same time, contrary to the views of Gall, a great many physiologists agree in thinking that the gray matter is the active part in the functions attributed to the nervous system, and that the white fibres are in the brain as in the

nerves, inert conductors. It still remains to determine which of these substances is pre-existent to the other; in what order the development of the different parts which they compose takes place, and what are the most essential differences in the nervous systems in different animals. It is sufficient to mention the name of Tiedemann to remind you of the light which has been thrown on these interesting and important questions.

I will not undertake to enumerate the many points which are yet to be elucidated. I will merely, before exposing my own views on the structure of the encephalon, allude to those of the most eminent anatomists who have attempted to determine the disposition of the two substances composing the encephalic masses.

Willis, Malpighi, Reil, Gall, and Spurzheim are of opinion that the white fibres of the crura cerebri, after emerging from the medulla oblongata, penetrate into the hemispheres, diverging in various directions till they meet the gray matter of the convolutions in which they terminate. They believe that the corpus callosum reaching from one hemisphere to the other is formed by the union on the median line of a new order of fibres, originating in that same gray matter of the convolutions, as that in which the fibres proceeding from the crura cerebri terminate. Gall clearly and distinctly expresses this opinion, when he says that the brain is formed of two different orders of fibres, the one of diverging, the other of converging fibres.

Tiedemann does not admit these two orders of fibres. According to his views, the fibres meeting in the corpus callosum are a continuation of those which proceed from the crura cerebri after they have gone through the entire circumference of the hemispheres.

Which of these different views is consistent with the truth? Is there no other difficulty than the choice between them?

It is pretty generally admitted that the encephalic masses are, according to the poetic language of Reil, "an efflorescence of the spinal marrow," the cerebrum being developed at the summit of its anterior, the cerebellum, at the summit of its posterior columns. The decussation between the two anterior pyramids belonging to the anterior columns is held to account for the effects of cerebral complaints being manifested on the side of the body opposed to their seat in the brain.

For this last fact to be true, it is necessary that the decussation between the two pyramids should be at once in communication with all the muscular nerves in the body and with

all the parts of the brain, which being altered produce symptoms on the opposite side. Hence in our survey the necessity of tracing accurately the connexions between the pyramids and the other parts of the cerebro-spinal system.

The spinal marrow situated below the decussation of the pyramids is composed of two symmetrical portions, which are united by a commissure of white matter. Without entering into the consideration of how many distinct columns are to be found in each half, I will now point out the different appearances belonging to the anterior and posterior surfaces of its commissure.

Throughout its whole extent the anterior commissure is divided longitudinally by numerous small holes, which are never to be found absolutely on the median line, where there exists a kind of very small raphe. At its superior extremity this anterior surface of the commissure becomes more superficial, and gradually assumes something of the aspect belonging to the decussation of the pyramids; so much so, that it is not always easy to determine where the commissure ends and the decussation begins.

The posterior surface of this commissure, which is visible at the bottom of the posterior fissure, does not present the same appearance as the anterior surface. The two surfaces are so near each other that they seem at first to belong to the same fibrous layer; but a close examination demonstrates that this is not the case, for there is constantly a sensible interval between the two surfaces. On the median line, two small very fine parallel white layers pass from the one to the other. If this double commissure be compared to the corpus callosum and fornix, these white layers going from the one to the other bear some analogy to the septum lucidum in the brain.

I think this cavity has already been noticed by Malpighi; and Dr. Hodgkin believes that he has more than once observed it. Gall described a longitudinal cavity on each side of the spinal cord, but it has been ascertained that they were produced by blowing into the central longitudinal masses on each side of the marrow. Thus we find in the commissure two transverse bands of white matter uniting the two halves of the spinal marrow, which bands are connected together on the median line by a delicate double band of white matter between the layers, of which a natural cavity seems to exist.

Let us now inquire what becomes of the two commissures when they penetrate on each side into the substance of the marrow. It seems to me that they soon become united together so as to constitute a kind of axis, around which are attached the lateral fibrous columns by means of fibres ra-

diating transversely from the former into the latter. It appears also that a greater number of the fibres going to the roots of the spinal nerves proceed directly from the axis itself. This distribution is more evident with respect to the posterior than to the anterior roots.

Without attempting to describe the lateral columns of the spinal marrow, I must not omit to mention a remarkable difference between the lateral parts of these columns and the anterior and posterior portions. If a portion of the spinal marrow be cut out transversely, freed from its membranes, and left to macerate in plain water for a few hours, the lateral parts of each half will assume, in consequence of the swelling of their fibrous fasciculi, an appearance something like that of a large nerve, whilst the anterior and posterior parts adjoining these lateral columns are uniformly swelled without presenting any such appearance. Let us now see how the axes of the superficial columns of the spinal marrow are connected with the parts developed at their upper extremity, that is to say, in the medulla oblongata.

The anterior part of the axis, in other words, the anterior part of the commissure, appears to go almost entirely into the decussation of the anterior pyramids, and into the pyramids themselves. A very fine commissure between the pyramids seems to be a continuation of the anterior commissure which exists throughout the whole length of the spinal marrow.

The posterior commissure ascending along the medulla oblongata comes very near to the surface, and when examined between the two lateral portions of the medulla, it seems to constitute a very delicate bond of union between the two sides of the calamus scriptorius. This union seems to be established by means of two slight fibrous columns, which are prolongations of the posterior commissure of the spinal marrow.

If these statements are correct, we see that when the prolongations of the commissures or naked surfaces of the axis of the spinal marrow are examined in the medulla oblongata, instead of being very near each other on the median line, they are separated by all the interval existing between the anterior and posterior surfaces of the medulla oblongata. But so slight are the remains of the commissures when examined in the medulla oblongata, that it is very easy to overlook them. When the two halves of the medulla oblongata are separated, and this separation is easily produced by introducing and gently pressing a probe between the two anterior pyramids or the two halves of the calamus scriptorius, they present to our view fibres going from before backwards, and so disposed, that those on the right side correspond to the intervals

of those on the left. On each side they seem to pass from the pyramid before to some small columns which proceed from the posterior commissure behind. These two opposed layers of fibres passing from before to behind, correspond in the medulla oblongata to the double band having the same disposition in the medulla spinalis.

With the posterior bundles of the medulla oblongata, continuous with the posterior commissure of the spinal marrow, are connected those white fibres seen on the surface of the calamus scriptorius, which are said to go to the auditory nerve.

I must not forget to say, that from these same posterior ascending bundles proceed radiating fibres, going to the superficial columns of the medulla oblongata, in like manner as there are found radiating fibres proceeding from the axis of the medulla spinalis and going to its superficial columns.

These remarkable anatomical arrangements are not to be found in the spinal marrow and medulla oblongata alone; they exist still higher, in the crura cerebri. I must not now attempt to describe these connexions further, as I have several other points to notice, and shall therefore be glad if I have succeeded in conveying to you some idea of their character. Nor will I here undertake to describe the multifarious and highly complicated parts which are to be found in and above the medulla oblongata going to the cerebellum and cerebrum, into which many of them may be followed. I will merely remark, that among all these fibrous bundles those in communication with the anterior pyramids are the most simple and the most direct in their progress to the brain through the crus cerebri, for they constitute the inferior part.

This inferior part of the crus cerebri, flattened a little transversely at its entrance into the brain, is received into a transverse cavity.

The superior part of the same crus cerebri when entering the brain unites directly with the thalamus nervi optici, which seems to be an enlargement and modification of this part. When attentively examined, the thalamus nervi optici is seen forming with its appendages, of which the nervus opticus emerging from it is the most remarkable, a complete circle, or at least a rounded mass, about the inferior part of the crus at its entrance into the brain.

It seems highly important to distinguish with exactness the further disposition of these two distinct prolongations of the crus cerebri.

Internal to the fissure of Sylvius there is an elongated four-sided surface, of whitish colour, which is the only fibrous part of the external surface of the brain. This quadrangle is

perforated by a great number of vascular holes, and its greatest dimension is from within outwards and backwards.

Its anterior margin is curved, and bears the external root of the olfactory nerve.

Its posterior margin, equally curved, bears a part of the optic nerve, which is prolonged outwards and backwards from the commissure.

At the anterior border of the same quadrangle is seen the base of the convolution which is in relation with the olfactory nerve.

To the extreme limit of its posterior margin is attached in some sort by its base the great tuberosity of the most internal convolution of the temporal lobe of the brain. This convolution is not less remarkable for its form and situation than for its surface, which appears invested by a whitish layer of matter perforated with holes like lace.

Internally this quadrangle is inclined towards the inferior part of the septum lucidum, and presents a swelling corresponding to the commissure of the optic nerves.

With the fibres composing the surface of this quadrangle are united all the roots of the olfactory nerve, except the anterior one, which is continued into the substance of the convolution on which this nerve rests.

With the fibres of the same quadrangle are also combined a layer of nervous matter and some radicular prolongations given off from the optic nerve.

The most internal portion of the quadrangle goes up under the septum lucidum to the base of the anterior part of the lateral ventricle. Its external portion, combined with the great tuberosity of the convolution of the cornu ammonis, or the convolution displaying the hippocampus major on its ventricular surface, corresponds with the deepest part of the descending cornu of the same ventricle in the temporal lobe.

By its adherent surface this quadrangle covers a part of the inferior extra-ventricular portion of the corpus striatum and thalamus nervi optici, and also receives a delicate layer of fibres which come from the corpus striatum and optic thalamus, in a line as distinct from that by which the fibrous planes proceed from the same parts to the internal surface of the convolutions, as that in which the anterior roots of the spinal nerves originate is distinct from that in which the posterior roots of the same nerves arise. Several radicular prolongations of the olfactory nerve go into the slight fibrous layer which is observable in the substance of the corpus striatum.

The anterior boundary of the fibrous quadrangle whose relations I have just now sketched, sends off a fibrous band,

which connected at its origin with the base of the convolution of the olfactory nerve, ascends in front of the corpus callosum, to the side of which it is applied, winds round its anterior extremity, then follows its superior aspect, and descending behind its posterior border into the convolution which bounds the fissure of Bichat, returns to the tuberosity situated on the external edge of the quadrangle so often mentioned.

This fibrous band therefore, with the quadrangle in which it ceases at its two opposite extremities, describes a large circle in which the corpus callosum is inscribed. The circumference of this fibrous band forms the base of a convolution whose remarkable disposition has engaged the attention of anatomists. It forms with the latter, on the edge of the corpus callosum contiguous to the hemispheres and the fissure of Bichat, a sort of listing, or border, in which the gray substance of the convolutions ends. I have thought it proper in an extensive work on the anatomy of the brain to name this band cerebral border, and the corresponding convolution, convolution of the border.

Thus, without multiplying details, we find reaching all along the circumference, or if the term which I have proposed be approved of, along the contracted border forming the limit of the extensive gray membrane folded into convolutions on the surface of the brain, a fibrous cord, which forms along with the superficial quadrangle whence it is derived, a complete circle round the corpus callosum, and this quadrangle and its immediate dependence communicate on the other side with the sensorial nerves of the brain.

Let us now take a rapid view of the fibrous parts of the hemispheres. They originate externally in a fasciculated fibrous layer interposed between the gray masses of the corpus striatum and optic thalamus, and go to the internal surface of the cerebral convolutions, whilst the fibrous layer whence they issue is continued into the inferior part of the crus cerebri, and consequently into the anterior pyramids. On the outside of the corpus striatum and optic thalamus, these fibrous planes of the hemisphere separate into two strata, upon which the cerebral convolutions are raised; and each of these strata or secondary planes becomes united to the external margin of the circular band forming the border of the hemispheres. One of these unions takes place in the hollow of the fissure of Sylvius; the other in all the rest of the internal circumference or border of the gray substance of the convolutions.

Of these two orders of fibres thus differently disposed, the

one then communicates with the posterior part of the crus cerebri and the nerves of sensation; the other with the inferior part of the crus cerebri and the anterior pyramids, which decussate like the effects of cerebral lesions bearing upon the powers of motion. The first order of fibres are applied to the circumference of the convolutions very near the median line, and the circle which they form is on a vertical plane, directed antero-posteriorly; the second, radiating from within outwards towards the middle of the convex surface of the hemispheres, spread themselves out from thence towards the termination of the convolutions round the corpus callosum, and become connected here with the external margin of the circular band, which extends to the same point of the convolutions.

This remark on the relation of the convolutions to the two orders of fibrous parts, of which the one proceeds from the anterior pyramids and the anterior parts of the spinal marrow, whilst the other is intimately connected with the sensorial nerve of the brain, and the posterior parts of the spinal marrow, points out the manner in which the cerebral convolutions should be studied.

If our description is confined to their forms, nothing of importance will be revealed. But if on the contrary we endeavour accurately to determine their relation to the other parts of the system by means of the fibrous parts entering into their composition, the mind will be prepared to apprehend the different offices they are intended to fulfil. This is what I have endeavoured to accomplish in the first place in the human brain, and secondarily in that of a considerable number of mammiferous animals. I shall subjoin here an abstract of my researches on this subject.

Considered in reference to the different connexions of their fibrous parts, the convolutions may be distinguished into two principal classes: first, those clothing the prolongations in the brain of the fibrous bands connected with the olfactory lobes, optic nerves, and posterior parts of the medulla spinalis; and second, those enveloping the cerebral terminations of the pyramidal fasciculus of the crus.

The convolutions connected with the prolongations in the brain of the fibrous parts which proceed from the basilar quadrangle, or the common meeting-point of the sensorial nerves and the posterior parts of the medulla, constitute all the plain internal part of the hemisphere, the surface of the basilar cerebello-temporal zone, and the lobule of the insula. Those developed on the terminations of the pyramidal fasciculus of the crus, constitute all the external portion of the

hemisphere and the concave surface of the orbital region of its base. The respective limits of these two classes of convolutions are indicated on the one hand by a grand line of convolutions which courses along in its whole extent the large circumference of the hemisphere, commencing in front at the anterior margin of the perforated quadrangle, and terminating behind at the posterior margin of the same quadrangle; and on the other hand by another line of convolutions, which forms the inclosure of the fissure of Sylvius, arising before and ending behind like the preceding grand line at the opposite margins of the perforated quadrangle.

Thus contiguous and united at their extremities, these two lines diverge from each other throughout the rest of their extent, the grand line traversing successively the internal border of the orbital region of the base of the brain, the great convex border of the hemisphere, and lastly, the external border of the basiliary cerebello-temporal zone; whilst the convolutions of the inclosure of the fissure of Sylvius travels in succession along the anterior border, the superior border, and lastly, the inferior border of the fissure which it bounds.

All the principal divisions found by each of these two lines of convolutions correspond to those formed by the other. That part of the large circumference which forms the internal border of the orbital region corresponds to the convolutionary inclosure of the fissure of Sylvius, forming the external limit of the same orbital region. The great convex boundary of the hemisphere corresponds to the superior border of the fissure of Sylvius; and lastly, the external border of the cerebello-temporal zone answers to the inferior border of the fissure of Sylvius. The anterior angle formed by the union of the first and second parts of the inclosure of the fissure of Sylvius, answers to the angle formed at the anterior extremity of the brain by the union of the great convex border of the hemisphere with the external border of the orbital region. The posterior angle of the fissure of Sylvius, subtended by the line which forms its superior border, meeting with that which forms its inferior border, answers to the angle formed by the meeting of the great convex border of the hemisphere with the external border of the cerebello-temporal zone at the posterior extremity of the brain. Lastly, as these two lines meet at their extremities, in contact with the perforated quadrangle, it is seen that the convolutions included in the interval between them occupying all the convex external aspect of the hemisphere and the orbital region of its inferior aspect, are quite distinct from those situated at the internal aspect

and at the basilar surface; and lastly, from those which in the fissure of Sylvius itself constitute the lobule of the insula. Of the convolutions of the regions which I have just pointed out, those of the internal aspect of the cerebello-temporal zone are included between the line of convolutions which tracks the large circumference of the hemisphere, and that part of the convolution of the border which extends from the anterior to the posterior margin of the perforated quadrangle, following the curve from before to behind of the corpus callosum, and the fissure of Bichat. Lastly, the convolutions of the insula are included between the line of convolution forming the inclosure of the fissure of Sylvius, and that part of the convolution of the border, very short in man, on which is prolonged the external root of the olfactory nerve.

If we dissect the convolutions of the plain internal surface of the hemisphere, those of the cerebello-temporal zone, and lastly, those of the insula, we can easily show that their fibrous parts converge from the great circumference of the hemisphere, and of the convolutionary inclosure of the fissure of Sylvius towards the corresponding regions of the convolution of the border, and terminate at last in the fibrous band of the border itself, whose connexions with the perforated quadrangle, the sensorial nerves of the brain, and the posterior parts of the spinal marrow we are already acquainted with. On the contrary, dissection of the convolutions on the external convex surface of the hemisphere included between the two grand lines of convolution which bound this aspect, one of these lines traversing the great circumference of the brain, the other following and forming the boundary of the fissure of Sylvius, shows that the fibrous twig of these convolutions terminates in the plane of the hemisphere which emanates from the pyramidal portion of the crus cerebri.

As to the two lines of convolution situated on the limit of those convolutions which are united with the dependences of the border and of those which envelope the terminations of the plane of the hemisphere, they both pertain by one of their margins, to the productions of the border, and by the other to the productions of the plane of the hemisphere. They form then a means of union between the two orders of convolutions, of which the one is connected to the sensorial nerves and the posterior parts of the spinal marrow, and the other to the anterior pyramids and the anterior parts of the spinal marrow.

Thus all the convolutions developed in the interval of the two spaces between the external surface of the hemisphere

and the internal and external margins of the orbital region, are in exclusive relation with the prolongations of the pyramidal fasciculus of the crus.

All the convolutions developed between the border, properly speaking, and the great circumference of the hemisphere, pertain to the fibrous emanations from the perforated quadrangle.

All the convolutions of the insula situated between the short region of the border which is traversed by the external root of the olfactory nerve, and the convolutionary inclosure of the fissure of Sylvius, belong peculiarly to the fibrous parts emanating from that region of the border to which belongs the external root of the olfactory nerve. Lastly, the great circumference in which are found the external surface of the convolutions belonging in particular to the terminations of the pyramidal fasciculus of the crus, and the internal surface of those which belong exclusively to the fibrous emanations from the perforated quadrangle, is destined by its double relations to a mixed purpose. It contains the anastomosis of the fibrous extremities of the pyramids and of the fibrous extremities issuing from the perforated quadrangle.

The convolutionary inclosure of the fissure of Sylvius, to the extra fissural margin of which are connected the convolutions developed upon the pyramidal termination of the crus, and with the fissural edge of which is combined the fibrous duplicature of the convolutions of the insula proceeding from that part of the border to which belongs the external root of the olfactory nerve, is destined, like the line of convolution which courses along the great circumference of the hemisphere, to contain the anastomosis of the fibrous parts belonging to the pyramidal convolutions with those which proceed from the border. They are thus in relation with the sensorial nerves of the brain, with the posterior parts of the crura, and with the medulla oblongata.

We might proceed to describe the various groups formed by the two orders of convolutions which we have now established; but it must not be forgotten that our present object is merely to give a succinct idea of the principal results of our anatomical investigations.

But I have as yet only pointed out a very trifling portion of the fibrous layers intermediate between the nerves of sensation of the brain, and I shall immediately proceed to mention the other parts which unite with the fibrous band and the superficial quadrangle previously described. Each of these parts forms, like the preceding, a circle which traverses the inferior part of the crus cerebri, some embracing this part

of the crus cerebri like a kind of bracelet, others separating from it in the greater part of their course, but always coming back to unite at their termination with the white quadrangle of the fissure of Sylvius.

Setting off from the crus, the first of these parts is the white superficial covering of the optic thalamus, the fibres of which are circular.

The second is the *tænia semicircularis*, incomplete as a ring when considered alone, but completed by the quadrangle with which it directly unites in front, whilst behind it joins the great tuberosity of the convolution of the cornu ammonis, attached to the external part of the superficial quadrangle.

Thirdly, the corpus striatum itself forms by its gray matter alone a complete circle, or rather an ellipse, the inferior extra-ventricular part of which is covered by the quadrangle so often mentioned.

Fourthly, to the outer side of the corpus striatum there is a fibrous circle, which surrounds it as the *tænia semicircularis* compasses the optic thalamus. So far as I know, this fibrous ring has not hitherto been described.

Fifthly, the corpus fimbriatum and the corresponding half of the fornix form likewise a complete circle with the fibrous layer of the fissure of Sylvius. In the sixth place would come, if we had not before described it, the white band of the border.

In the seventh and last place, the two little bands situated upon the corpus callosum close to the median line, terminate, like all the preceding parts, at the anterior and posterior limits of the white perforated surface internal to the fissure of Sylvius.

The corpus callosum itself is in intimate relation with all these concentric circles. Its particular disposition was described in a memoir which I had the honour of reading before the Academy of Medicine at Paris in 1825; but the death of Beclard, who was appointed to report on my paper, deprived me of the judgement which that illustrious professor would have formed.

There is not, I believe, any communication between the fibrous expansions of the inferior part of the crura of the opposite sides of the brain.

It appears to me that in man the anterior commissure only unites those parts of the opposite sides which are connected with the nerves of sensation. The well-known fact that in many mammifera this commissure only stands on each side to the olfactory nerves instead of reaching from one hemisphere to the other, adds force to the opinion I express, that

the anterior commissure only unites parts devoted to sensation.

I shall not insist further on this anatomical proposition, but if you will allow me briefly to add a few physiological deductions, I would say that the fibrous parts intermediate between the internal surface of the convolutions and the anterior pyramids, appear to me to be simple conductors, as well as those which unite the organs of sensation with the circumference of the gray substance of the convolutions; these transmitting to the muscles the influence of the gray substance which determines their contraction, whilst those convey from the organs of sense to the same gray substance impressions made on the surface of these organs.

The grey substance of the convolutions appears to me to be the material substratum, by the intervention of which the will directs the movements of the body. For the last twenty years lesions of this substance have been pointed to as those most frequently occurring in the insane, by those physicians who expect to find in the brains of such patients alterations corresponding to the characteristic symptoms of their disorders.

Atrophy of the convolutions, so frequently seen in dementia, appears to me to result from disuse of the functions of the gray substance; then the fibrous matter proceeding from this gray substance becomes atrophied also, just as the optic nerves fall into a state of atrophy in the blind.

Pathological anatomy furnishes numerous examples of lesions of the fibrous matter intermediate between the cortical substance of the convolutions and the anterior pyramids. Paralysis of the active organs of motion on the opposite side is generally the consequence.

The information afforded by pathological anatomy, relative to the effects of lesions affecting the fibrous parts intermediate between the gray substance of the convolutions and the organs of sense, is not so clear. This is owing, I imagine, to the communication in the median line between these fibrous parts of opposite sides, and the great number of these parts rendering the complete obstruction of the impressions they are meant to convey, difficult.

Before summing up these conclusions, relative to the structure and office of some parts of the brain, I should like to say a few words on the relations which the study of the cranium establishes between different regions of this bony case and the corresponding regions of the organ which it incloses.

If the two frontal eminences are divided horizontally by sawing on a plane perpendicular to their centre, and this sec-

tion is carried to some depth in the brain, the lateral ventricles are opened by their anterior extremities, each of these extremities terminating in a cul-de-sac, answering to a frontal eminence. In the interval between these two cul-de-sacs of the anterior extremities of the ventricles we reach the anterior curve of the corpus callosum. If we saw in the same way the two superior occipital protuberances, we arrive at the posterior extremity of the two lateral ventricles, each of these extremities ending in a hollow cone, answering to one of the superior occipital depressions and eminences. Lastly, if the saw is made to divide the two parietal eminences at their summits, and that portion of the long vault intermediate between them, it leads to that part of the lateral ventricles which is the most spacious and projects the most outwards. It is in this part of the ventricles that is situated, so to speak, the confluence of the anterior, posterior, and temporal regions of these cavities. The same section which leads from the parietal eminences to that part of the lateral ventricles which is the most spacious and projects the most outwards, falls in the interval of the two eminences and of the corresponding portion of the ventricles upon the posterior margin of the corpus callosum.

There exists on a level with the squamous portion of the temporal bone a depression in the interior of the cranium and a corresponding eminence, almost as considerable, on the outside. If we cut through this eminence and the contiguous part of the brain at the same time, we open the base of the temporal region of the ventricle.

Now as there is nothing on the surface of the brain to account for the cranial prominences of which I have just spoken, it appears to me that we may very fairly consider them as caused by the shape of the corresponding regions of the ventricles. This conclusion is strengthened by comparing the form of these eminences with that of the portions of the ventricles corresponding to them. The frontal eminences are round, like the two cul-de-sacs forming the anterior extremities of the ventricles. The occipital protuberances, and especially the depressions answering to them in the interior of the skull, are sharper, if this language is applicable to protuberances and depressions; and the greater acuteness of the posterior extremities of the ventricles in relation with these protuberances, is a fact sufficiently notorious. Lastly, the temporal eminences are oblique in the same direction as the corresponding part of the hollow of the ventricles. But the influence of the ventricular cavities, or of the serous sacs of the brain, is not confined to the formation of the different

pairs of eminences which I have just pointed out; it extends also to the form of the skull in general, which, in fact, it contributes to determine.

The different transverse sections, which I have supposed to be made on a level with the centre of all these pairs of eminences on the median zone of the cranial vault, divide this median zone into four regions, always perceptible during life, and each of them presenting an angular curve on the transverse line which separates it from the neighbouring regions. All of them display too an agreement in form and proportions with the corresponding region of the envelope of the ventricles, which seems to me incontestable.

The first region, comprised between the frontal eminences and the inferior boundary of the forehead, answers exclusively to the convolutions developed in front of and beneath the corpus callosum, and presents but a small projection from above downwards, like the corresponding part of the ventricles. The lower projection on this part of the forehead does not always indicate a considerable development of the corresponding part of the brain. It may, in fact, be simply owing to the great size of the frontal eminences, and in this case the fact may be ascertained by percussion.

The second region, included between the frontal and parietal eminences, always forms the largest division of the median zone of the cranial vault; it is arched like the corpus callosum itself, and corresponds to the convolutions above this body. Its size, compared with that of the other regions of the bony arch, bears the same proportion to theirs, that the extent of the superior part of the corpus callosum does to the other fibrous parts enveloping the serous cavities of the brain.

A considerable eminence is very often seen on the median line towards the centre of this region, in the upper part of the os frontis. This also appears to me, like all the other projections on the median line, to be owing to a thickening of the bones.

The third region, intermediate between the transverse section of the parietal eminences and the superior occipital protuberances, is scarcely ever convex longitudinally. It is mostly straight or concave in this direction. Frequently even it presents on the median line a well-marked fossa. The complete separation for the two hemispheres at this part, the absence of the corpus callosum, lifted up as in the others by the fluid of the ventricles, and the superior concavity observed in passing from those parts of the ventricles covered by the corpus callosum, to those formed by a distinct fibrous cone

in each posterior half of the hemispheres, are in accordance with these peculiarities. To this region correspond the convolutions situated behind the posterior margin of the corpus callosum as far as the posterior termination of the ventricles.

Lastly, the fourth region, situated in the interval between the superior occipital protuberances and the upper curved line of the occiput, divides the latter into a median convex quadrangle, and two lateral triangles, whose pointed summits terminate near the mastoid process. Now these triangles, instead of being convex like the rest of the median arch, almost always present a plain or even concave surface. The depressed portions of these triangles correspond at their sharp summits to the insertion of the tentorium cerebelli and the external part of the lambdoidal suture.

In the temporal regions of the cranial arch, we always remark, on a level with the great wing of the sphenoid bone, a depression running upwards and backwards in the same direction as the fissure of Sylvius, to which it corresponds. A right line drawn from the top of this depression towards the centre of the parietal eminence, marks the course of the fissure of Sylvius, and allows us to measure, on the living subject, the comparative volume of cerebral substance situated in front, and of that behind, this fissure.

Now the fissure of Sylvius, and the cranial depression answering to it at the fore part of the temporal fossa, are variously modified according to the modifications of the ventricular hollow. The anterior and temporal regions of these cavities are separated by a large nervous mass which follows the crus cerebri. The fore part of the ventricle is enlarged above this mass and the temporal part of the ventricle below it.

The fissure of Sylvius, then, forms the interval between two regions of the ventricle, the frontal and the temporal, and consequently the interval between the convolutions covering in these two distinct regions of the serous cavity. It ceases above, at the part where these regions of the ventricular cavity unite into a common conflux. Its deepest part is below, at the bottom of the widest interval between the frontal and temporal extremities of the ventricle.

Thus the form of the brain, and that of the skull also, would seem to be determined in their general character by the form of the serous sacs inclosed in the hemispheres, and constantly filled with the fluid peculiar to them.

This is not meant to imply that the cerebral convolutions do not exert any influence on the secondary shape of the

skull. So far from that, observation proves that when the convolutions are greatly developed, the skull, though retaining the form that we have assigned to it, swells out in the intervening regions between the eminences corresponding to the extremities of the ventricles, so as almost to obliterate them; or if they still remain very prominent, they acquire a very great diameter. In this case, the head, modified by the development of the convolutions, acquires the cerebral form, *par excellence*.

On the contrary, when the convolutions are but very little developed, the prominences, and the regions separating them, are all exceedingly well marked, whilst, at the same time the projections corresponding to the ventricular extremities have a very small diameter. The head then assumes, if I may be allowed to use such a term, the peculiarly ventricular form.

The relations just now pointed out between the projecting points of the skull and the corresponding regions of the ventricles, appear still further confirmed by observing what takes place in chronic hydrocephalus. One of the first symptoms of this dropsy of the ventricles is the increased salience of these cranial tumours. But I shall not pursue the details of the relations between the skull and the brain any further at present. I consider them useful in reference to what is called surgical anatomy, and also for all those cases which call for an exact knowledge of the relations between the skull and the brain. The subject has been pursued in a work of some extent, which I intend before long to lay before the public.

To sum up, I consider that the fibrous parts of the brain are conductors; some from without to within, others from within to without. I believe that these conducting parts may be distinguished into afferentes and efferentes, and that the distinct course of both the one and the other may be demonstrated. The first are inserted especially into the circumference of the gray substance, and the second into its internal surface.

The gray substance of the convolutions intermediate between the two preceding orders of fibrous parts, seems to me to be the material substratum, through the instrumentality of which the will directs the movements of the body.

The prominences constantly seen coupled in pairs on the arch of the skull appear to me to be produced by the projection of the corresponding regions of the ventricles. The median eminences, not universally present, appear to be produced by a thickening of the bones.

The median zone of the cranial arch is naturally divided into four sections; one, anterior, corresponding to the fore part of the corpus callosum, and to the convolutions de-

veloped before and beneath the level of the same part of that body.

The second, more extensive, intermediate between the frontal and parietal eminences, is of a length proportioned to the extent from before to behind of the corpus callosum. It ceases behind on a level with the posterior margin of that body.

The third section, often concave from above to below, sometimes even hollowed into a furrow on the median line, is proportionate in length to that of that part of the hemispheres completely separated, behind the corpus callosum.

The fourth section, intermediate between the superior occipital protuberances and the upper curved line of the os occipitis, displays in its middle a projecting quadrangle, corresponding to the hinder extremities of the ventricles, and to the convolutions situated behind and beneath these extremities*.

March 30, 1840.

V. *Remarks on the permanent Soap Film and on Thin Plates.*
By JOSEPH READE, M.D.

“Scilicet ut possem curvo dignoscere rectum,
Atque inter silvas Academi quærere verum.”—HORACE.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

AS the entire of the second book of Newton's Optics is based on the theory first advanced by Dr. Hook, or rather by Mr. Boyle, “that colours are produced by the thicknesses of the plates,” supposing your scientific readers well acquainted with that great philosopher's experiments, I shall immediately proceed with my own. Should these be capable of explanation according to the Newtonian theory, I shall be ever ready to alter my opinions, and what at present I conceive to be legitimate inferences. Aware of the difficulties I have to encounter, and of the prejudices against everything opposed to a doctrine stamped with the name of Newton, and advocated by the most celebrated characters, I hope for the indulgence of my scientific readers.

Experiment 1. Having made a permanent soap film, as already described in your Journal †, and shown two years ago at the British Association at Liverpool, I placed the bottle on an inclined plane on the table until all the bands of colours

* An able report on the subject of Dr. Foville's researches has recently been presented to the Academy of Sciences by Professor Blainville. It contains further discoveries made by the Doctor with regard to the origin of the eighth pair of nerves.

† [Lond. and Ed. Phil. Mag. vol. xi. p. 375.—EDIT.]

had evaporated and the entire film become of a black appearance, what Newton likened to a hole in his bubble, transmitting almost the entire light. I now moved the bottle to and from me, and in a short time the entire film was clothed with silvery white, reflecting atoms, which soon formed into beautiful bands of colours by means of cohesive attraction; for, as they formed nearly simultaneously, they could not be produced by a descending fluid, causing relative thicknesses; but perhaps it may be said, that the film was thickened by evaporation of the saponaceous fluid. To obviate this objection, I made the following new, and I presume to hope, conclusive experiment.

Experiment 2. Having procured a cylindrical glass tube, about one inch in diameter, I dipped one end to the depth of three inches into a saponaceous solution and formed a film, which, when the tube was held perpendicularly, glided down three inches. I now corked the upper orifice, to prevent further descent, and laying the tube on the table, in a short time the bands were formed; when these disappeared, and the film was black, I shook the tube from side to side, and in a short time the black changed to a silvery white, and then formed into bands of different colours. On placing it again on the table the film appeared all over black. Here there could be no thickening of the plates by evaporation, as the air was at the bottom of the tube. This experiment, easy of manipulation, I hope may be esteemed conclusive, or, as Sir Isaac Newton calls it, an "*experimentum crucis*." As my experiments are now shown in lecture-rooms, I am anxious that they should be accompanied by legitimate inferences, particularly as some observations made by a Dublin Professor at Liverpool, prevented an explanation which I now give.

Experiment 3. Having procured a plate of very deep blue glass, four inches square, I wiped it well, and then breathed on it through a narrow glass tube, forming a plate of vapour, which, by evaporation, went through all the relative thicknesses measured by Newton, without any variety of colours. I now breathed a second time on this plate, and drew my finger across the middle so as to make furrows in the plate of vapour; immediately all the variety of colours in nature was formed, like threads of variegated silk. This experiment evidently shows that the atoms were relatively approximated, and that it was this approximation, and not any relative thickness of the plate of vapour, which caused the colours.

Experiment 4. On repeating the experiments of the Abbé Mazéas, "*Mémoires présentes*," with two pieces of plane glass, he justly remarks, that friction is necessary in the formation of

coloured rings. Now it is evident that friction cannot alter the thickness of a plate of air, but must bring into view some substances capable of condensation; and there is nothing mixed with the air capable of such condensation, except vapour. I therefore must attribute the colours to this cause. Convinced that vapour was the cause, I improved on the Abbé Mazéas's experiment: for, on washing one of the plates of glass with soap and water, and holding it before the fire, then breathing on the other, by means of a slight degree of friction I coloured the entire glass, with a large black spot in the centre more than an inch in diameter. When this black spot was formed, the glasses were so firmly united as to require a strong force to separate them. This I attribute to the air being pressed out, and then the glasses acted in the same manner as a leather soaker used by school-boys, and not to any cohesive attraction of the surfaces, as supposed by chemists. I coated the glasses with a plate of water, and on pressing them together no colours appeared, until by a strong degree of friction I produced vapour. I also smeared the glasses with some candle-grease, and found no colours, until I held the glasses to the fire, when, on using friction, vapour and colours were produced.

Atomic Theory of Colours.

Having endeavoured to prove, I hope with effect, that colours are not produced by relative thicknesses of the plate, as advanced by Sir Isaac Newton in the second book of his *Optics*, I shall proceed to give what I conceive is the true explanation of this interesting phænomenon. Grimaldi and others maintained that light was capable of condensation and rarefaction. However, as they brought forward no experiments to prove it, I think it unfair to say that I took my theory from that celebrated philosopher. If we hold a candle before a black shade made with a pencil or any other slender and opaque substance, and hold the paper sideways to the window, two shadows are formed, the one blue from the candle, the other brown from the daylight. Now this brown shadow can be changed to an orange by approximating the candle; at a yet nearer distance the orange becomes a perfect yellow, and when very close the colour entirely disappears, or the light of the shadow becomes rarefied into perfect transparency. The blue in like manner undergoes rarefaction and change of colour, from a blue to a purple; and when the candle is very near the coil of paper, the shadow becomes black, because then there is but one light, that of the sun. To argue that the shadow is a mere privation, would be to say that brown, orange and yellow, blue and purple were privations:

(green was made by overlapping the blue and yellow shadows.) Surely, if it were a mere privation, the light of the candle could only illuminate the black shadow and make it white. However, the fact is, that the black shadow is condensed light, and rarefied into different colours. If we suppose a number of grains of shot to represent the soap atoms, placed at different distances from one another, and represented by a , a^2 , a^3 , a^4 , &c., &c., the light passes through those and is reflected from the second surface, and gives, when variously condensed, this or that colour, according to the approximation of these atoms, and not the thickness of the plates*.

Ascending and Descending Currents in a Soap Film.

On making a soap film I placed it on an inclined plane, and perceived the coloured bands to descend slowly, and increasing in breadth, until at last the attraction of cohesion lost its influence, and the atoms became free and ascended in currents, particularly at the concave sides of the bottle; as they rose generally white atoms, they passed through the coloured bands, until after passing through blue, red, green, &c. they fell into the ranks of their own colour. In boiling water we perceive currents by means of powdered resin, or other light substances, evidently caused by an addition of caloric. Here there is no such addition, and we must look to some other cause, perhaps electricity. Sometimes these atoms take an elliptical or circular motion; this is best seen by placing the bottle with the plane film on the table and surrounding it with the warm hand; as the room was at 60° and my hand at 80° , I threw 20° into the film. Here there can be no differences of thickness; however, almost simultaneously, the force of cohesive attraction forms the coloured bands, and when the film becomes white, then these beautiful and interesting movements take place. How far these laws may act on the solar system, I leave to the contemplation of the astronomer. All fluids are in perpetual motion, from the broad Atlantic to the permanent soap film; and hereafter I shall be enabled, by some new experiments, to show that the same laws regulate the atmosphere.

Colours of the Clouds and Complementary Colours.

When the soap film was entirely black, after remaining perhaps an hour on the table, I placed the bottle in a basin of boiling water, and in a short time perceived the film to be

* Count Rumford, as well as others, made many experiments on coloured shadows, but entirely overlooked these changes; at last, in despair, he says, it was a *deceptio oculi*.

clothed with white reflecting atoms; some of these soon changed to a purple, blue, orange, &c., and when the bottle was placed on an inclined plane the coloured bands were formed, broad and vivid. To ascertain whether the bands of colour were similar by transmission and reflection, I held the film above a lighted candle, at an angle of about 45° , and perceived exactly the same coloured bands, and not any complementary colours, "*magnis componere parva.*" We must attribute the colours of the clouds to the approximation of vaporific atoms, and not to different-sized globules of water. It is an interesting object of inquiry, to say why the black reflecting atoms at great distances, should by heat or agitation, approximate to form white or bands of colour. We may, hypothetically, surmise that these atoms are spherical, and are enlarged by caloric, in the same manner as air in a balloon or bladder; or we may suppose that, by agitation, &c., they become oblate spheroids.

Nothing is more surprising than the permanency of this soap film. In a few seconds Newton's bubble bursts, and consequently hindered him from making deliberate observations; but now that we have one which lasts for months, can be washed and renewed, an inferior mind may investigate the phenomena of light and colours with more success than even that great philosopher, in the infancy of experimental science. Indeed, every new experiment opens a wider field for research, and I am sanguine enough to think that the permanent soap film and the newly invented iriscope may produce interesting discoveries. With intent to ascertain this permanency, I formed two films with a similar saponaceous mixture, in equally-sized bottles; the one was corked, the other open, and consequently exposed to the action of the atmosphere; in a short time the open one broke, the other remained for weeks. I therefore inferred that the film broke from the vaporific action of the atmosphere. That it was not from pressure, I proved, by putting in a very long cork, so that the pressure might be increased; nor was it from agitation, as supposed by Newton, for in washing the film it was well shaken; neither could it proceed from any chemical action, as I made the film *in vacuo*. I remarked that a film formed on the mouth of a wine glass remained a long time when the atmosphere was heavy and charged with vapour, whereas of a dry day it speedily burst. I hence infer that the air in a corked bottle becomes saturated with vapour, and thus that evaporation is diminished. I shall now sum up the foregoing arguments, to show that the colours do not proceed from relative thicknesses.

1. The bands form simultaneously, or nearly so.

2. Plates of grease, water, vapour, go through all the thicknesses without colours; but as soon as friction forms vapour, or the finger approximates the atoms, colours appear.

3. By holding the bottle to the fire, or even by the heat of the hand, circular currents of red, white, blue, rise in a green field; and, at all times, coloured atoms are both ascending and descending, contrary to gravity and thickness, the bands increase in breadth as they descend.

4. To end. I have succeeded in making a film sufficiently thick, with soap, as to counteract cohesive attraction, yet sufficiently thin to evaporate without undergoing any change of colour.

The following diagrams give the different stages of the atomic theory of colours.

Fig. 1.

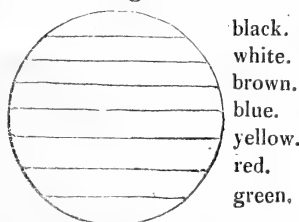


Fig. 2.

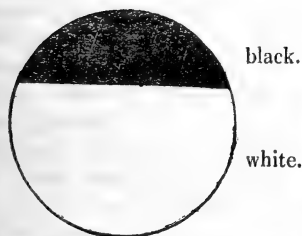


Fig. 3.



In fig. 2, how can different thicknesses produce three-fourths of the film white? In fig. 3 there is a very small segment of white at the bottom, and by shaking the bottle the entire film becomes white, and then forms into chromatic bands. The best method of seeing these minute saponaceous atoms forming colours, is to shake fig. 2, when atoms no larger than the point of a pin are diffused in the black, and after a time are seemingly dissolved in the black atoms, just as when a lump of white sugar is dissolved in hot water; at first the solution is clouded, and then the saccharine atoms

become as minute as those of the water, and equally semi-transparent. A candle held before fig. 3 is faintly reflected from the second surface, as there is no actual contact of atoms in nature; all substances, even the hardest metals, are porous.

In my next communication I shall give a theory of the Iriscope, compared with Nobili's rings.

I remain, Gentlemen,

Your obedient servant,

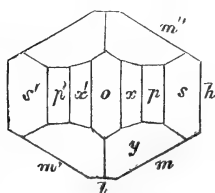
London, June 8, 1840.

J. READE, M.D.

VI. On the Form and Optical Constants of Nitre. By
Professor MILLER*.

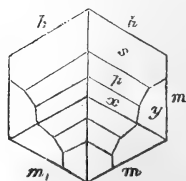
THE following values of the angles between normals to the several faces of nitrate of potash are calculated from means of the best measurements of a large number of crystals at a temperature of about 19° centigrade. The close agreement of the results afforded by different crystals, renders it probable that the errors of the concluded angles do not in any case exceed half a minute.

Fig. 1.



$l o$	$90^{\circ} 0'$
$o h$	$90 \quad 0$
$h l$	$90 \quad 0$
$m m'$	$61 \quad 10$
$x x'$	$38 \quad 38$

Fig. 2.



$p p'$	$70^{\circ} 31.5$
$s s'$	$109 \quad 0$
$h y$	$65 \quad 41$
$l y$	$45 \quad 50$
$o y$	$54 \quad 1.5$

In twin crystals having the twin axis perpendicular to one of the faces m (fig. 2).

$$m m_1 \quad 57^{\circ} 40' \quad h h_1 \quad 61^{\circ} 10'.$$

The parameters are respectively proportional to the numbers 2.4285; 1.4352; 1.7023.

The cleavage most easily obtained is parallel to the faces p . When the temperature of the crystal is increased 100° C.

* Communicated by the Author.

the angle between normals to the faces $p p'$ increases about $44'$: $m m'$ does not perceptibly change.

The symbols of the simple forms are

$$\begin{array}{llll} h \{100\}, & l \{010\}, & o \{001\}, & y \{111\}, \\ p \{101\}, & s \{201\}, & x \{102\}, & m \{110\}. \end{array}$$

No other forms were observed.

The optic axes lie in a plane parallel to the face l , and, in air, appear to make with each other an angle of $8^\circ 40'$. The minimum deviations of the brightest rays of the spectrum refracted through the faces m, m'' are $24^\circ 15'$ and $38^\circ 49'$, for light polarized in planes respectively perpendicular and parallel to the intersection of the faces m, m'' . Hence the velocity of the brightest rays of the spectrum in air divided by the velocities within the crystal, will be 1.5052, 1.5046, and 1.333, for rays in planes parallel to h, l, o , polarized in those places respectively.

St. John's College, Cambridge, June 4th, 1840.

VII. *Observations on Mr. Smith's Experiments on Fermentation.* By R. RIGG, F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE reading of Mr. Denham Smith's paper printed in the Number of your valuable Periodical for March, entitled, "Observations on the supposed Formation of Inorganic Elements during Fermentation," induces me to ask you the favour of saying, through the same publication, that in the abstract of my paper on that subject, which is noticed by that gentleman, the increase in the quantity of earthy and alkaline matter therein mentioned, does not apply to the original quantity contained in both the refined sugar and the yeast employed in my experiments, but to that in the former only. The latter previous to using being deprived of its soluble matter by repeated washings in cold water, loses very little, if any, of its solid materials when employed in promoting the vinous fermentation.

It may be added that the circumstances which I have found most favourable for the formation of these bodies during this chemical action on refined sugar, are when the sugar at the commencement forms about 20 per cent. of the weight of the mixture, and when about nine-tenths of this dissolved body has undergone decomposition by a quick process, the

experiments being kept under slight pressure and at an equable temperature of near 70° Fahr.

Mixtures of this kind conducted through the vinous fermentation in silver and china vessels always afforded a larger quantity of ashes after evaporation and calcination than those made in glass apparatus. But upon the proportionate increase by this process in any experiment, we can speak at the outset with as little certainty as upon the height to which any plant will grow when the seed is first planted. One of the features which I have always found to accompany their formation is an increase in colour, which colour again disappears when they undergo decomposition by the putrefactive fermentation and have ammonia as a product.

I have found the quantity to vary with almost every circumstance, even in experiments made with the same mixture, from little or no increase to that of twenty times the weight of those included in the sugar experimented upon. My experiments made with solutions containing about ten per cent. of their weight of sugar (the strength employed by Mr. Smith) rarely gave more than an appreciable increase upon the original quantity, except when the fermentation was conducted very quickly.

The ashes were obtained by evaporating and burning over a spirit-lamp without the addition of any foreign agent, the materials being in an open platinum vessel. Their quantity was determined by weight as a whole, with the addition of their power of neutralizing acids as regards their alkaline property.

Walworth Road, April 14th, 1840.

VIII. *On Increasing the Light of a common Argand Lamp.*
By C. HOLTHOUSE, Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

HAVING seen in the Number of your Magazine of March last, a letter from Sir J. Herschel, "On a simple mode of obtaining from a common Argand Oil Lamp a greatly increased quantity of Light," I take the liberty of sending you a few observations of my own on the same subject; they are mostly corroborative of what has been advanced by the distinguished philosopher before alluded to, and if you think them worth insertion they are very much at your service.

* Communicated by the Author.

The size and brilliancy of the flame in a common Argand oil lamp depend chiefly upon the shape and dimensions of the glass chimney, and its position in relation to the flame; but as the contrivances which render the latter more brilliant at the same time diminish its bulk, there is a limit, beyond which we cannot increase the brightness without diminishing the illuminating power; the increase in brilliancy not compensating for the diminished bulk of the flame. Sir J. Herschel has not told us what is the diameter of his Argand burner, or the diameter and form of his glass chimney; but from the description he has given us of his method of producing a greatly increased quantity of light, I presume he must have used one of those chimneys which are commonly employed only for gas lamps, being a simple cylinder about nine inches long, and of equal diameter throughout. But this kind of chimney is ill adapted for enlivening a flame, and seems merely to serve for protecting it against currents of air, although, when employed in the manner recommended by Sir J. Herschel, its office is reversed; it becomes a mean of increasing the intensity of combustion, but serves no longer as an efficient protector: and in this latter respect it is imperfect; for admitting the advantages which arise from the increase of light, still these can only be available while a lamp is at rest: the instant it is moved the flame must necessarily impinge against the walls of the chimney, at the great risk of breaking it, and with a certainty of smoking it. But an equal degree of light, and not subject to the inconvenience just mentioned, can be obtained by one of the chimneys ordinarily made use of in the common table lamp, which consists of a base or larger cylinder, upon which the pillar or smaller cylinder is joined by a horizontal part, termed the shoulder: upon the height of the pillar of this chimney, the proportion of its constituent parts to each other and to the burner, and their position in relation to the latter, depend the efficient burning of the lamp.

And first with reference to the *height*. The most obvious effect of lengthening a chimney is, to render the flame more flickering and unsteady, at the same time its brightness is slightly increased, and its bulk diminished: the larger the diameter of the chimney compared with the burner, the less marked are these changes. On lengthening a chimney whose pillar had a diameter of $1\frac{1}{8}$ inch, to three feet, and placing it on a lighted lamp, well turned on, whose wick had $\frac{7}{8}$ of an inch diameter, the flame immediately split into several small flickering cones, the largest of which was not above five or six lines in height; as the chimney was shortened, so did the

flame in the same proportion become steadier and more uniform, till at seven inches, which was the length of the pillar of this chimney, it was as steady as could be desired. One fact was strikingly obvious during this experiment, viz., that the increase of brilliancy was by no means commensurate with the loss of light occasioned by the smaller bulk and great unsteadiness of the flame. From several trials with chimneys of various lengths, I should say that a seven inch pillar, for an Argand burner of the ordinary size, is well adapted for giving brightness, and at the same time steadiness, to a flame.

To ascertain the best *diameter* for the chimney, eight pieces of tin were procured, each two inches square, and having a circular aperture in their centre, varying from one and a half to half an inch in diameter; these, by turns, were fixed to the moveable stage of a microscope, and then placed directly over the flame of a lamp, so that the centre of the circle of the Argand burner should correspond with the centre of the circular aperture in the tin; a simple* glass chimney being now placed upon the tin, the whole apparatus was lowered till the flame of the lamp passed through the aperture in the tin. By trying the different sizes one after the other, we were able to decide upon that which gave the greatest degree of illumination, and by raising and lowering the apparatus over the flame, the best height from the level of the wick for obtaining the greatest light was determined. The following are some of the results. The size of the flame was in a direct ratio with the size of the aperture in the tin, but its brilliancy was in an inverse ratio; in other words, the flame diminished in size but increased in brilliancy as the aperture through which it had to pass was lessened. The effect produced by chimneys of different diameters, but of equal length, provided the aperture in the tin over which they were placed remained the same, was so trifling, that it was scarcely appreciable: the diameter of the aperture in the tin being, for instance, one inch, and that of the chimney the same, no material alteration in the brilliancy or bulk of the flame was produced when a chimney of double the diameter was substituted for it. I need hardly observe, that the diameter of the chimney must not be less than that of the tin. I said at the commencement of this letter, that there was a limit beyond which we could not increase the bright-

* When I apply the term *simple* to a chimney, I wish to be understood as speaking of those cylinders of uniform diameter from end to end; those composed of two parts, I shall, to prevent unnecessary tautology, designate *compound*.

ness of the flame without diminishing its illuminating power. I am not at present prepared to say where this precise point is; but I can assert, without fear of contradiction, that an aperture of one and one-eighth of an inch diameter, for a lamp whose wick is seven-eighths of an inch, is a size well calculated for giving a great degree of brilliancy to the flame, without materially diminishing its bulk. We come now to the consideration of the best height for placing the plate of tin above the wick of the lamp; this appears to be from two-eighths to three-eighths of an inch, which is that specified by Sir J. Herschel, supposing his burner to have an inch diameter: when we go below this the flame becomes brighter, less coniform, and shorter; the continuity of its circle is interrupted; chasms form in it, producing the appearance of so many distinct flames; and when the tin has reached the level of the wick, the lamp is well nigh extinguished. Sir J. Herschel, in describing the effect produced on the flame by raising and lowering the chimney, has suggested that it would be an improvement were our common lamps provided with a mechanism for this purpose; this has actually been done on the continent, and in this country by Mr. Samuel Parker of Piccadilly, the flame of whose hot-oil lamp is regulated entirely by raising and lowering the chimney, the wick remaining stationary. Now if a number of glass chimneys be made, the diameter of whose pillars shall be the same as those of the apertures in the tin, and these be suspended over the flame with their narrow end downwards, we shall have the same result: but if, instead of this, they be placed in the usual manner on their support, the large end lowermost, we shall still have precisely the same result, provided the commencement of the pillar bear the same position, in reference to the wick, as the upper end did when suspended over it. The diameter of one inch and an eighth, which I have named as a good size for having the pillar of the chimney, is eligible likewise on another account: there is less danger of its becoming smoked, to which it would be very liable were it of smaller dimensions than that just specified. The base of the chimney may be an inch and three-quarters or two inches in diameter, and its shoulder should form a right angle with the level of the burner, about half an inch above it. This is a form of chimney which I have proved to be well fitted for giving a degree of light very superior to what is seen in Argand lamps having the ordinary sized chimneys. Dr. Ure, in a very interesting paper read before the Institution of Civil Engineers in June last, seems to give the preference to chimneys with rounded shoulders; these certainly give greater steadiness to the flame,

and if the light is equal to that afforded by the rectangular shouldered ones, they should be preferred.

One more point, which was barely touched upon by Sir J. Herschel, yet remains to be decided: Is this superior splendour of the flame attended by an increased consumption of oil? If we reasoned from analogy, we should certainly reply in the affirmative; intensity of combustion being ever attended with corresponding consumption of the combustible material. But to place this beyond a doubt, the following experiment was undertaken. Two Argand lamps, having wicks and chimneys of the same diameter and length, and furnished with the same kind of oil, were placed in a pair of scales, each at an equal distance and elevation from the centre of a sheet of white paper pasted on the wall. On being lighted, the flames were regulated till the shadows cast by a small ruler had an equal depth; when this had been fairly determined both by myself and others, weights were put into the stand containing the lighter lamp, till the two exactly balanced each other. The chimney was now quickly removed from one of the lamps, and another substituted for giving a clear white flame, the time being at the same moment noted: they were allowed to burn for forty-two minutes, and were then simultaneously extinguished. At the expiration of this time, the lamp burning with the bright flame had consumed 100 grains more than the other. The experiment being repeated, but with smaller flames, the increased consumption of oil in the bright burning lamp was 50 grains. Whether this greater expenditure of oil is balanced by the increased degree of illumination, and the solution of some other interesting questions connected with this subject, I shall reserve till a future period.

I am, Gentlemen,

Your obedient Servant,

13, Keppel Street, Russell Square,
April 11, 1840.

C. HOLTHOUSE.

IX. *A Letter to Prof. Faraday, on certain Theoretical Opinions.* By R. HARE, M.D., Professor of Chemistry in the University of Pennsylvania*.

DEAR SIR,

1. I HAVE been indebted to your kindness for several pamphlets comprising your researches in electricity, which I have perused with the greatest degree of interest.

* From Silliman's American Journal of Science and Arts, Vol. 38, No. I. [We have taken the liberty of numbering the paragraphs of Dr. Hare's letter.—ED.]

2. You must be too well aware of the height at which you stand, in the estimation of men of science, to doubt that I entertain with diffidence any opinion in opposition to yours. I may say of you as in a former instance of Berzelius, that you occupy an elevation inaccessible to unjustifiable criticism. Under these circumstances, I hope that I may, from you, experience the candour and kindness which were displayed by the great Swedish chemist in his reply to my strictures on his nomenclature.

3. I am unable to reconcile the language which you hold in paragraph 1615, with the fundamental position taken in 1165. Agreeably to the latter, you believe ordinary induction to be the action of *contiguous* particles, consisting of a species of polarity, instead of being an action of either particles or masses at "*sensible distances*." Agreeably to the former, you conceive that "assuming that a perfect vacuum was to intervene in the course of the line of inductive action, it does not follow from this theory that the line of particles on opposite sides of such a vacuum would not act upon each other." Again, supposing "it possible for a positively electrified particle to be in the centre of a vacuum an inch in diameter, nothing in my present view forbids that the particle should act at a distance of half an inch on all the particles forming the inner superficies of the bounding sphere."

4. Laying these quotations before you for reconsideration, I beg leave to inquire how a positively excited particle, situated as above described, can react "inductrically" with any particles in the superficies of the surrounding sphere, if this species of reaction require that the particles between which it takes place be contiguous. Moreover if induction be not "an action either of particles or masses at *sensible distances*," how can a particle situated as above described, "*act at the distance of half an inch on all the particles forming the disk of the inner superficies of the bounding sphere?*" What is a sensible distance, if half an inch is not?

5. How can the force thus exercised obey the "well-known law of the squares of the distances," if as you state (1375) the rarefaction of the air does not alter the intensity of the inductive action? In proportion as the air is rarefied, do not its particles become more remote?

6. Can the ponderable particles of a gas be deemed contiguous in the true sense of this word, under any circumstances? And it may be well here to observe, that admitting induction to arise from an affection of intervening ponderable atoms, it is difficult to conceive that the intensity of this af-

section will be inversely as their number as alleged by you. No such law holds good in the communication of heat. The air in contact with a surface at a constant elevation of temperature, such for instance as might be supported by boiling water, would not become hotter by being rarefied, and consequently could not become more efficacious in the conduction of heat from the heated surface to a colder one in its vicinity.

7. As soon as I commenced the perusal of your researches on this subject, it occurred to me that the passage of electricity through a vacuum, or a highly rarefied medium, as demonstrated by various experiments, and especially those of Davy, was inconsistent with the idea that ponderable matter could be a necessary agent in the process of electrical induction. I therefore inferred that your efforts would be primarily directed to a re-examination of that question.

8. If induction, in acting through a vacuum, be propagated in right lines, may not the curvilinear direction which it pursues, when passing through "dielectrics," be ascribed to the modifying influence which they exert?

9. If, as you concede, electrified particles on opposite sides of a vacuum can act upon each other, wherefore is the received theory of the mode in which the excited surface of a Leyden jar induces in the opposite surface a contrary state, objectionable?

10. As the theory which you have proposed, gives great importance to the idea of polarity, I regret that you have not defined the meaning which you attach to this word. As you designate that to which you refer, as a "species of polarity," it is presumable that you have conceived of several kinds with which ponderable atoms may be endowed. I find it difficult to conceive of any kind which may be capable of as many degrees of intensity as the known phenomena of electricity require; especially according to your opinion that the only difference between the fluid evolved by galvanic apparatus and that evolved by friction, is due to opposite extremes in quantity and intensity; the intensity of electrical excitement producible by the one being almost infinitely greater than that which can be produced by the other. What state of the poles can constitute quantity—what other state intensity, the same matter being capable of either electricity, as is well known to be the fact? Would it not be well to consider how, consistently with any conceivable polarization, and without the assistance of some imponderable matter, any great difference of intensity in inductive power can be created?

11. When by friction the surface is polarized so that particles are brought into a state of constraint from which they

endeavour to return to their natural state, if nothing be superadded to them, it must be supposed that they have poles capable of existing in two different positions. In one of these positions, dissimilar poles coinciding, are neutralized; while in the other position, they are more remote, and consequently capable of acting upon other matter.

12. But I am unable to imagine any change which can admit of gradations of intensity, *increasing* with remoteness. I cannot figure to myself any reaction which increase of distance would not lessen. Much less can I conceive that such extremes of intensity can be thus created, as those of which you consider the existence as demonstrated. It may be suggested that the change of polarity produced in particles by electrical inductions, may arise from the forced approximation of reciprocally repellent poles, so that the intensity of the inductive force, and of their effort to return to their previous situation, may be susceptible of the gradation which your electrical doctrines require. But could the existence of such a repellent force be consistent with the mutual cohesion which appears almost universally to be a property of ponderable particles? I am aware that, agreeably to the ingenious hypothesis of Mossotti*, repulsion is an inherent property of the particles which we call ponderable; but then he assumes the existence of an imponderable fluid to account for cohesion; and for the necessity of such a fluid to account for induction it is my ultimate object to contend. I would suggest that it can hardly be expedient to ascribe the phænomena of electricity to the polarization of ponderable particles, unless it can be shown, that if admitted, it would be competent to produce all the known varieties of electric excitement, whether as to its nature or energy.

13. If I comprehend your theory, the opposite electrical state induced on one side of a coated pane, when the other is directly electrified, arises from an affection of the intervening vitreous particles, by which a certain polar state caused on one side of the pane, induces an opposite state on the other side. Each vitreous particle having its poles severally in opposite states, they are arranged as magnetized iron filings in lines; so that alternately opposite poles are presented in such a manner that of all one kind are exposed at one surface, and all of the other kind at the other surface. Agreeably to this or any other imaginable view of the subject, I cannot avoid considering it inevitable that each particle must have at least two poles. It seems to me that the idea of polarity

* [See Scientific Memoirs, vol. i., p. 448.—EDIT.]

requires that there shall be in any body possessing it, two opposite poles. Hence you correctly allege that agreeably to your views it is impossible to charge a portion of matter with one electric force without the other. (See par. 1177.) But if all this be true, how can there be a "positively excited particle?" (See par. 1616.) Must not every particle be excited negatively, if it be excited positively? Must it not have a negative, as well as a positive pole?

14. I cannot agree with you in the idea, that consistently with the theory which ascribes the phænomena of electricity to one fluid, there can ever be an isolated existence either of the positive or negative state. Agreeably to this theory, any excited space, whether minus or plus, must have an adjoining space relatively in a different state. Between the phænomena of positive and negative excitement there will be no other distinction than that arising from the direction in which the fluid will endeavour to move. If the excited space be positive, it must strive to flow outward; if negative, it will strive to flow inward. When sufficiently intense, the direction will be shown by the greater length of the spark, when passing from a small ball to a large one. It is always longer when the small ball is positive, and the large one negative, than when their positions are reversed*.

15. But for any current it is no less necessary that the pressure should be on one side, comparatively minus, than that on the other side it should be comparatively plus; and this state of the forces must exist whether the current originates from a hiatus before, or from pressure behind. One current cannot differ essentially from another, however they may be produced.

16. In paragraph 1330, I have been struck with the following query, "What then is to separate the principle of these extremes, perfect conduction and perfect insulation, from each other; since the moment we leave the smallest degree of perfection at either extremity, we involve the element of perfection at the opposite ends?" Might not this query be made with as much reason in the case of motion and rest, between the extremes of which there is an infinity of gradations? If we are not to confound motion with rest, because in proportion as the former is retarded, it differs less from the latter; wherefore should we confound insulation with conduction, because in proportion as the one is less efficient, it becomes less remote from the other?

* See my Essay on the causes of the diversity in the length of the sparks, erroneously distinguished as positive and negative, in vol. v. American Philosophical Transactions.

17. In any case of the intermixture of opposite qualities, may it not be said in the language which you employ, "the moment we leave the element of perfection at one extremity, we involve the element of perfection at the opposite"? Might it not be said of light and darkness, or of opakeness and translucency? in which case, to resort to your language again, it might be added, "especially as we have not in nature, a case of perfection at one extremity or the other." But if there be not in nature any two bodies, of which one possesses the property of perfectly resisting the passage of electricity, while the other is endowed with the faculty of permitting its passage without any resistance; does this affect the propriety of considering the qualities of *insulation* and conduction in the abstract, as perfectly distinct, and inferring that so far as matter may be endowed with the one property, it must be wanting in the other?

18. Have you ever known electricity to pass through a pane of sound glass? My knowledge and experience create an impression that a coated pane is never discharged through the glass unless it be cracked or perforated. That the property by which glass resists the passage of electricity, can be confounded with that which enables a metallic wire to permit of its transfer, agreeably to Wheatstone's experiments, with a velocity greater than that of the solar rays, is to my mind inconceivable.

19. You infer that the residual charge of a battery arises from the partial penetration of the glass by the opposite excitements. But if glass be penetrable by electricity, why does it not pass through it without a fracture or perforation?

20. According to your doctrine, induction consists "in a forced state of polarization in contiguous rows of the particles of the glass" (1300); and since this is propagated from one side to the other, it must of course exist equally at all depths. Yet the partial penetration suggested by you, supposes a collateral affection of the same kind, extending only to a limited depth. Is this consistent? Is it not more reasonable to suppose that the air in the vicinity of the coating gradually relinquishes to it a portion of free electricity, conveyed into it by what you call "*convection*." The coating being equally in contact with the air and glass, it appears to me more easy to conceive that the air might be penetrated by the excitement, than the glass.

21. In paragraph 1300, I observe the following statement: "*When a Leyden jar is charged, the particles of the glass are forced into this polarized and constrained condition by the* Phil. Mag. S. 3. Vol. 17. No. 107. July 1840. E

electricity of the charging apparatus. Discharge is the return of the particles to their natural state, from their state of tension, whenever the two electric forces are allowed to be disposed of in some other direction." As you have not previously mentioned any particular direction in which the forces are exercised during the prevalence of this constrained condition, I am at a loss as to what meaning I am to attach to the words "some other direction." The word *some*, would lead to the idea that there was an uncertainty respecting the direction in which the forces might be disposed of; whereas it appears to me that the only direction in which they can operate, must be the opposite of that by which they have been induced.

22. The electrified particles can only "return to their natural state" by retracing the path by which they departed from it. I would suggest that for the words "*to be disposed of in some other direction,*" it would be better to substitute the following, "*to compensate each other by an adequate communication.*"

23. Agreeably to the explanation of the phænomenon of coated electrics afforded in the paragraph above quoted (1300), by what process can it be conceived that the opposite polarization of the surfaces can be neutralized by conduction through a metallic wire? If I understand your hypothesis correctly, the process by which the polarization of one of the vitreous surfaces in a pane produces an opposite polarization in the other, is precisely the same as that by which the electricity applied to one end of the wire extends itself to the other end.

24. I cannot conceive how two processes severally producing results so diametrically opposite as insulation and conduction, can be the same. By the former, a derangement of the electric equilibrium may be permanently sustained, while by the other, all derangement is counteracted with a rapidity almost infinite. But if the opposite charges are dependent upon a polarity induced in contiguous atoms of the glass, which endures so long as no communication ensues between the surfaces; by what conceivable process can a perfect conductor cause a discharge to take place, with a velocity at least as great as that of the solar light? Is it conceivable that all the lines of "contra-induction" or depolarization can concentrate themselves upon the wire from each surface so as to produce therein an intensity of polarization proportioned to the concentration; and that the opposite forces resulting from the polarization are thus reciprocally compensated? I must confess, such a concentration of such forces or states, is to me difficult to reconcile with the conception that it is at all to be ascribed to the action of rows of *contiguous ponderable particles*.

25. Does not your hypothesis require that the metallic particles, at opposite ends of the wire, shall in the first instance be subjected to the same polarization as the excited particles of the glass; and that the opposite polarizations, transmitted to some intervening point, should thus be mutually destroyed, the one by the other? But if discharge involves a return to the same state in vitreous particles, the same must be true in those of the metallic wire. Wherefore then are these dissipated, when the discharge is sufficiently powerful? Their dissipation must take place either while they are in the state of being polarized, or in that of returning to their natural state. But if it happen when in the first-mentioned state, the conductor must be destroyed before the opposite polarization upon the surfaces can be neutralized by its intervention. But if not dissipated in the act of being polarized, is it reasonable to suppose that the metallic particles can be sundered by returning to their *natural state* of depolarization?

26. Supposing that ordinary electrical induction could be satisfactorily ascribed to the reaction of ponderable particles, it cannot, it seems to me, be pretended that magnetic and electro-magnetic induction is referable to this species of reaction. It will be admitted that the Faradian currents do not for their production require intervening ponderable atoms.

27. From a note subjoined to page 37 of your pamphlet, it appears that "on the question of the existence of one or more imponderable fluids as the cause of electrical phenomena, it has not been your intention to decide." I should be much gratified if any of the strictures in which I have been so bold as to indulge, should contribute to influence your ultimate decision.

28. It appears to me that there has been an undue disposition to burden the matter, usually regarded as such, with more duties than it can perform. Although it is only with the properties of matter that we have a direct acquaintance, and the existence of matter rests upon a theoretical inference that since we perceive properties, there must be material particles to which those properties belong; yet there is no conviction which the mass of mankind entertain with more firmness than that of the existence of matter in that ponderable form, in which it is instinctively recognised by people of common sense. Not perceiving that this conviction can only be supported as a theoretic deduction from our perception of the properties; there is a reluctance to admit the existence of other matter, which has not in its favour the same instinctive conception, although theoretically similar reasoning would apply. But if one kind of matter be admitted to exist because

we perceive properties, the existence of which cannot be otherwise explained, are we not warranted, if we notice more properties than can reasonably be assigned to one kind of matter, to assume the existence of another kind of matter?

29. Independently of the considerations which have heretofore led some philosophers to suppose that we are surrounded by an ocean of electric matter, which by its redundancy or deficiency is capable of producing the phænomena of mechanical electricity, it has appeared to me inconceivable that the phænomena of galvanism and electro-magnetism, latterly brought into view, can be satisfactorily explained without supposing the agency of an intervening imponderable medium by whose subserviency the inductive influence of currents or magnets is propagated. If in that wonderful reciprocal reaction between masses and particles, to which I have alluded, the polarization of condensed or accumulated portions of intervening imponderable matter, can be brought in as a link to connect the otherwise imperfect chain of causes; it would appear to me a most important instrument in lifting the curtain which at present hides from our intellectual vision, this highly important mechanism of nature.

30. Having devised so many ingenious experiments tending to show that the received ideas of electrical induction are inadequate to explain the phænomena without supposing a modifying influence in intervening ponderable matter, should there prove to be cases in which the results cannot be satisfactorily explained by ascribing them to ponderable particles, I hope that you may be induced to review the whole ground, in order to determine whether the part to be assigned to contiguous ponderable particles, be not secondary to that performed by the imponderable principles by which they are surrounded.

31. But if galvanic phænomena be due to ponderable (*imponderable*?) matter, evidently that matter must be in a state of combination. To what other cause than an intense affinity between it and the metallic particles with which it is associated, can its confinement be ascribed consistently with your estimate of the enormous quantity which exists in metals? If "a grain of water, or a grain of zinc, contain as much of the electric fluid as would supply eight hundred thousand charges of a battery containing a coated surface of fifteen hundred square inches," how intense must be the attraction by which this matter is confined? In such cases may not the material cause of electricity be considered as latent, agreeably to the suggestion of CErsted, the founder of electro-magnetism? It is in combination with matter, and only capable of producing the appro-

priate effects of voltaic currents when in act of transfer from combination with one atom to another; this transfer being at once an effect and a cause of chemical decomposition, as you have demonstrated.

32. If polarization in any form can be conceived to admit of the requisite gradations of intensity, which the phænomena seem to demand; would it not be more reasonable to suppose that it operates by means of an imponderable fluid existing throughout all space, however devoid of other matter? May not an electric current, so called, be a progressive polarization of rows of the electric particles, the polarity being produced at one end and destroyed at the other incessantly, as I understood you to suggest in the case of contiguous ponderable atoms.

33. When the electric particles within different wires are polarized in the same tangential direction, the opposite poles being in proximity, there will be attraction. When the currents of polarization move oppositely, similar poles coinciding, there will be repulsion. The phænomena require that the magnetized or polarized particles should be arranged as tangents to the circumference, not as radii to the axis. Moreover, the progressive movement must be propagated in spiral lines in order to account for rotary influence.

34. Between a wire which is the mean of a galvanic discharge and another not making a part of a circuit, the electric matter which intervenes may, by undergoing a polarization, become the medium of producing a progressive polarization in the second wire moving in a direction opposite to that in the inducing wire; or in other words an electrical current of the species called Faradian may be generated.

35. By progressive polarization in a wire, may not stationary polarization or magnetism be created; and reciprocally by magnetic polarity may not progressive polarization be excited?

36. Might not the difficulty, above suggested, of the incompetency of any imaginable polarization to produce all the varieties of electrical excitement which facts require for explanation, be surmounted by supposing intensity to result from an accumulation of free electric polarized particles, and quantity from a still greater accumulation of such particles, polarized in a latent state or in chemical combination?

37. There are it would seem many indications in favour of the idea that electric excitement may be due to a forced polarity, but in endeavouring to define the state thus designated, or to explain by means of it the diversities of electrical charges, currents and effects, I have always felt the incompetency of

any hypothesis which I could imagine. How are we to explain the insensibility of a gold-leaf electroscope, to a galvanized wire, or the indifference of a magnetic needle to the most intensely electrified surfaces?

38. Possibly the Franklinian hypothesis may be combined with that above suggested, so that an electrical current may be constituted of an imponderable fluid in a state of polarization, the two electricities being the consequence of the position of the poles, or their presentation. Positive electricity may be the result of an accumulation of electric particles, presenting poles of one kind; negative, from a like accumulation of the same matter with a presentation of the opposite poles, inducing of course an opposite polarity. The condensation of the electric matter, within ponderable matter, may vary in obedience to a property analogous to that which determines the capacity for heat, and the different influence of dielectrics upon the process of electrical induction may arise from this source of variation.

With the highest esteem, I am yours truly,
ROBERT HARE.

An Answer to Dr. Hare's Letter on certain Theoretical Opinions. By M. FARADAY.

MY DEAR SIR,

i. **Y**OUR kind remarks have caused me very carefully to revise the general principles of the view of *static induction* which I have ventured to put forth, with the very natural fear that as it did not obtain your acceptance, it might be founded in error; for it is not a mere complimentary expression when I say I have very great respect for your judgement. As the reconsideration of them has not made me aware that they differ amongst themselves or with facts, the resulting impression on my mind is, that I must have expressed my meaning imperfectly, and I have a hope that when more clearly stated my words may gain your approbation. I feel that many of the words in the language of electrical science possess much meaning; and yet their interpretation by different philosophers often varies more or less, so that they do not carry exactly the same idea to the minds of different men: this often renders it difficult, when such words force themselves into use, to express with brevity as much as, and no more than, one really wishes to say.

ii. My theory of induction (as set forth in Series xi. xii. and xiii.) makes no assertion as to the nature of electricity, or at all questions any of the theories respecting that subject

(1667). It does not even include the origination of the developed or excited state of the power or powers; but taking that as it is given by experiment and observation, it concerns itself only with the arrangement of the force in its communication to a distance in that particular yet very general phænomenon called *static induction* (1668.). It is neither the nature nor the amount of the force which it decides upon, but solely its mode of distribution.

iii. Bodies whether conductors or non-conductors can be *charged*. The word *charge* is equivocal: sometimes it means that state which a glass tube acquires when rubbed by silk, or which the prime conductor of a machine acquires when the latter is in action; at other times it means the state of a Leyden jar or similar inductive arrangement when it is said to be charged. In the first case the word means only the peculiar condition of an electrified mass of matter considered by itself, and does not apparently involve the idea of induction; in the second it means the whole of the relations of two such masses charged in opposite states, and most intimately connected by inductive action.

iv. Let three insulated metallic spheres A, B and C be placed in a line, and not in contact; let A be electrified positively, and then C uninsulated; besides the general action of the whole system upon all surrounding matter, there will occur a case of inductive action amongst the three balls, which may be considered apart, as the type and illustration of the whole of my theory: A will be charged positively; B will acquire the negative state at the surface towards A, and the positive state at the surface furthest from it; and C will be charged negatively.

v. The ball B will be in what is often called a polarized condition, i. e. opposite parts will exhibit the opposite electrical states, and the two sums of these opposite states will be exactly equal to each other. A and C will not be in this polarized state, for they will each be, as it is said, charged (iii.), the one positively, the other negatively, and they will present no polarity as far as this particular act of induction (iv.) is concerned.

vi. That one part of A is more positive than another part does not render it polar in the sense in which that word has just been used. We are considering a particular case of induction, and have to throw out of view the states of those parts not under the inductive action. Or if any embarrassment still arise from the fact that A is not uniformly charged all over, then we have merely to surround it with balls, such as B and C, on every side, so that its state shall be alike on every

part of its surface (because of the uniformity of its inductive influence in all directions) and then that difficulty will be removed. A therefore is charged, but not polarly; B assumes a polar condition; and C is charged inducteously (1483.), being by the prime influence of A brought into the opposite or negative electrical state through the intervention of the intermediate and polarized ball B.

vii. Simple charge therefore does not imply polarity in the body charged. Inductive charge (applying that term to the sphere B and all bodies in a similar condition (v.)) does (1672.). The word charge as applied to a Leyden jar, or to the *whole* of any inductive arrangement, by including *all* the effects, comprehends of course both these states.

viii. As another expression of my theory, I will put the following case. Suppose a metallic sphere C, formed of a thin shell a foot in diameter; suppose also in the centre of it another metallic sphere A only an inch in diameter; suppose the central sphere A charged positively with electricity to the amount we will say of 100; it would act by induction through the air, lac, or other insulator between it and the large sphere C; the interior of the latter would be negative, and its exterior positive, and the sum of the positive force upon the whole of the external surface would be 100. The sphere C would in fact be polarized (v.) as regards its inner and outer surfaces.

ix. Let us now conceive that instead of mere air, or other insulating dielectric, within C between it and A, there is a thin metallic concentric sphere B six inches in diameter. This will make no difference in the ultimate result, for the charged ball A will render the inner and outer surfaces of this sphere B negative and positive, and it again will render the inner and outer surfaces of the large sphere C negative and positive, the sum of the positive forces on the outside of C being still 100.

x. Instead of one intervening sphere let us imagine 100 or 1000 concentric with each other, and separated by insulating matter, still the same final result will occur; the central ball will act inductrically, the influence originating with it will be carried on from sphere to sphere, and positive force equal to 100 will appear on the outside of the external sphere.

xi. Again, imagine that all these spheres are subdivided into myriads of particles, each being effectively insulated from its neighbours (1679.), still the same final result will occur; the inductric body A will polarize all these, and having its influence carried on by them in their newly acquired state, will exert precisely the same amount of action on the external

sphere C as before, and positive force equal to 100 will appear on its outer surface.

xii. Such a state of the space between the inductric and inducteous surfaces represents what I believe to be the state of an insulating dielectric under inductive influence; the particles of which by the theory are assumed to be conductors individually, but not to one another (1669.).

xiii. In asserting that 100 of positive force will appear on the outside of the external sphere under all these variations, I presume I am saying no more than what every electrician will admit. Were it not so, then positive and negative electricities could exist by themselves, and without relation to each other (1169. 1177.), or they could exist in proportions not equivalent to each other. There are plenty of experiments, both old and new, which prove the truth of the principle, and I need not go further into it here.

xiv. Suppose a plane to pass through the centre of this spherical system, and conceive that instead of the space between the central ball A and the external sphere C being occupied by a uniform distribution of the equal metallic particles, three times as many were grouped in the one half to what occurred in the other half, the insulation of the particles being always preserved: then more of the inductric influence of A would be conveyed outwards to the inner surface of the sphere C, though that half of the space where the greater number of metallic particles existed, than through the other half: still the exterior of the outer sphere C would be uniformly charged with positive electricity, the amount of which would be 100 as before.

xv. The actions of the two portions of space, as they have just been supposed to be constituted (xiv.), is as if they possessed two different *specific inductive capacities* (1296.); but I by no means intend to say, that *specific inductive capacity* depends in all cases upon the number of conducting particles of which the dielectric is formed, or upon their vicinity. The full cause of the evident difference of inductive capacity of different bodies is a problem as yet to be solved.

xvi. In my papers I speak of all induction as being dependent on the action of contiguous particles, i. e. I assume that insulating bodies consist of particles which are conductors individually (1669.), but do not conduct to each other provided the intensity of action to which they are subject is beneath a given amount (1326. 1674. 1675.); and that when the inductric body acts upon conductors at a distance, it does so by polarizing (1298. 1670.) all those particles which occur in the portion of dielectric between it and them. I have used

the term *contiguous* (1164. 1673.), but have I hope sufficiently expressed the meaning I attach to it: first by saying at par. 1615, "the next existing particle being considered as the contiguous one;" then in a note to par. 1665, by the words, "I mean by contiguous particles those which are next to each other, not that there is no space between them;" and further by the note to par. 1164. of the octavo edition of my *Researches*, which is as follows: "The word contiguous is perhaps not the best that might have been used here and elsewhere, for as particles do not touch each other it is not strictly correct. I was induced to employ it because in its common acceptation it enabled me to state the theory plainly and with facility. By contiguous particles, I mean those which are next."

xvii. Finally, my reasons for adopting the molecular theory of induction were the phænomena of electrolytic discharge (1164. 1343.), of induction in curved lines (1166. 1215.), of specific inductive capacity (1167. 1252.), of penetration and return action (1245.), of difference of conduction and insulation (1320.), of polar forces (1665.), &c. &c., but for these reasons and any strength or value they may possess I refer to the papers themselves.

xviii. I will now turn to such parts of your critical remarks as may require attention. A man who advances what he thinks to be new truths, and to develope principles which profess to be more consistent with the laws of nature than those already in the field, is liable to be charged, first with self-contradiction; then with the contradiction of facts; or he may be obscure in his expression, and so justly subject to certain queries; or he may be found in non-agreement with the opinions of others. The first and second points are very important, and every one subject to such charges must be anxious to be made aware of, and also to set himself free from or acknowledge them; the third is also a fault to be removed if possible; the fourth is a matter of but small consequence in comparison with the other three; for as every man who has the courage, not to say rashness, of forming an opinion of his own, thinks it better than any from which he differs, so it is only deeper investigation, and most generally future investigators who can decide which is in the right.

xix. I am afraid I shall find it rather difficult to refer to your letter. I will, however, reckon the paragraphs in order from the top of each page, considering that the first which has its *beginning* first in the page*. In referring to my own mat-

* We shall change Prof. Faraday's references for the numbers which we have attached to Dr. Hare's letter, and refer thus, par. 23, &c.

ter I will employ the usual figures for the paragraphs of the Experimental Researches, and small Roman numerals for those of this communication.

xx. At paragraph 3, you say, you cannot reconcile my language at 1615, with that at 1165. In the latter place I have said I believe *ordinary induction* in all cases to be an action of *contiguous* particles, and in the former assuming a very hypothetical case, that of a vacuum, I have said nothing in my theory forbids that a charged particle in the centre of a vacuum should act on the particle next to it, though that should be half an inch off. With the meaning which I have carefully attached to the word *contiguous* (xvi.) I see no contradiction here in the terms used, nor any natural impossibility or improbability in such an action. Nevertheless all *ordinary induction* is to me an action of contiguous particles, being particles at insensible distances: induction across a vacuum is not an ordinary instance, and yet I do not perceive that it cannot come under the same principles of action.

xxi. As an illustration of my meaning, I may refer to the case, parallel with mine, as to the extreme difference of interval between the acting particles or bodies, of the modern views of the radiation and conduction of heat. In radiation the rays leave the hot particles and pass occasionally through great distances to the next particle, fitted to receive them: in conduction, where the heat passes from the hotter particles to those which are contiguous and form part of the same mass, still the passage is considered to be by a process precisely like that of radiation; and though the effects are, as is well known, extremely different in their appearance, it cannot as yet be shown that the principle of communication is not the same in both.

xxii. So on this point respecting contiguous particles and induction across half an inch of vacuum, I do not see that I am in contradiction with myself or with any natural law or fact.

xxiii. Paragraph 4 is answered by the above remarks and by viii. ix. x.

xxiv. Paragraph 5 is answered according to my theory by viii. ix. x. xi. xii. and xiii.

xxv. Paragraph 6 is answered, except in the matter of opinion (xviii.), according to my theory by xvi. The conduction of heat referred to in the paragraph itself will, as it appears to me, bear no comparison with the phænomenon of electrical induction:—the first refers to the distant influence of an agent which travels by a very slow process, the second to one where distant influence is simultaneous, so to speak, with the origin of the force at the place of action:—the

first refers to an agent which is represented by the idea of one imponderable fluid, the second to an agency better represented probably by the idea of two fluids, or at least by two forces:—the first involves no polar action, nor any of its consequences, the second depends essentially on such actions;—with the first, if a certain portion be originally employed in the centre of a spherical arrangement, but a small part appears ultimately at the surface; with the second, an amount of force appears instantly at the surface (viii. ix. x. xi. xii. xiii. xiv.) exactly equal to the exciting or moving force, which is still at the centre.

xxvi. Paragraph 13 involves another charge of self-contradiction, from which, therefore, I will next endeavour to set myself free. You say I “correctly allege that it is impossible to charge a portion of matter with one electric force without the other (see par. 1177). But if all this be true, how can there be a *positively excited particle*? (see par. 1616). Must not every particle be excited negatively if it be excited positively? Must it not have a negative as well as a positive pole?” Now I have not said exactly what you attribute to me; my words are, “it is impossible, experimentally, to charge a portion of matter with one electric force *independently* of the other: charge always implies *induction*, for it can in no instance be effected without (1177).” I can, however, easily perceive how my words have conveyed a very different idea to your mind, and probably to others, than that I meant to express.

xxvii. Using the word *charge* in its simplest meaning (iii. iv.), I think that a body *can* be charged with one electric force without the other, that body being considered in relation to itself only. But I think that such charge cannot exist without induction (1178.), or independently of what is called the development of an equal amount of the other electric force, not in itself, but in the neighbouring consecutive particles of the surrounding dielectric, and through them of the facing particles of the uninsulated surrounding conducting bodies, which, under the circumstances, terminate as it were the particular case of induction. I have no idea, therefore, that a particle when charged must itself of necessity be polar; the spheres A B C of iv., v., vi., vii., fully illustrate my views (1672.).

xxviii. Paragraph 20 includes the question, “is this consistent?” implying self-contradiction, which, therefore, I proceed to notice. The question arises out of the possibility of glass being a (slow) conductor or not of electricity, a point questioned also in the two preceding para-

graphs. I believe that it is. I have charged small Leyden jars made of thin flint glass tube with electricity, taken out the charging wires, sealed them up hermetically, and after two and three years have opened and found no charge in them. I will refer you also to Belli's curious experiments upon the successive charges of a jar and the successive return of portions of these charges*. I will also refer to the experiments with the shell lac hemisphere, especially that described in 1237. of my Researches; also the experiment in 1246. I cannot conceive how, in these cases, the air in the vicinity of the coating could gradually relinquish to it a portion of free electricity, conveyed into it by what I call convection, since in the first experiment quoted (1237.), when the return was gradual, there was *no coating*; and in the second (1246.), when there was *a coating*, the return action was most sudden and instantaneous.

xxix. Paragraphs 21 and 22 perhaps only require a few words of explanation. In a charged Leyden jar I have considered the two opposite forces on the inductric and inductive surfaces as being directed towards each other through the glass of the jar, provided the jar have no projection of its inner coating, and is uninsulated on the outside (1682.). When discharge by a wire or discharger, or any other of the many arrangements used for that purpose is effected, these supply the "some other directions" spoken of (1682. 1683.).

xxx. The inquiry in paragraph 23, I should answer by saying, that the process is the same as that by which the polarity of the sphere B (iv., v.,) would be neutralized if the spheres A and C were made to communicate by a metallic wire; or that by which the 100 or 1000 intermediate spheres (x.) or the myriads of polarized conducting particles (xi.) would be discharged, if the inner sphere A, and the outer one C, were brought into communication by an insulated wire; a circumstance which would not in the least affect the condition of the power on the exterior of the globe C.

xxxi. The obscurity in my papers, which has led to your remarks in paragraph 25, arises, as it appears to me (after my own imperfect expression), from the uncertain or double meaning of the word discharge. You say, "if discharge involves a return to the same state in vitreous particles, the same must be true in those of the metallic wire. Wherefore then are these dissipated when the discharge is sufficiently powerful?" A jar is said to be discharged when its charged

* *Bibliotheca Italiana*, 1837, lxxxv., p. 417.

state is reduced by any means, and it is found in its first indifferent condition. The word is then used simply to express the state of the apparatus; and so I have used it in the expressions criticised in paragraph 21, already referred to. The process of discharge, or the mode by which the jar is brought into the discharged state, may be subdivided, as of various kinds; and I have spoken of conductive (1320.), electrolytic (1343.), disruptive (1359.), and convective (1562.) discharge, any one of which may cause the discharge of the jar, or the discharge of the inductive arrangements described in this letter (xxx.), the action of the particles in any one of these cases being entirely different from the mere return action of the polarized particles of the glass of the jar, or the polarized globe B (v.), to their first state. My view of the relation of insulators and conductors, as bodies of one class, is given at 1320. 1675. &c. of the *Researches*; but I do not think the particles of the good conductors acquire an intensity of polarization anything like that of the particles of bad conductors; on the contrary, I conceive that the contiguous polarized particles (1670.) of good conductors discharge to each other when their polarity is at a very low degree of intensity (1326. 1338. 1675.). The question of why are the metallic particles dissipated when the charge is sufficiently powerful, is one that my theory is not called upon at present to answer, since it will be acknowledged by all, that the dissipation is not necessary to discharge. That different effects ensue upon the subjection of bodies to different degrees of the same power, is common enough in experimental philosophy: thus, one degree of heat will merely make water hot, whilst a higher degree will *dissipate* it as steam, and a lower will convert it into ice.

xxxii. The next most important point, as it appears to me, is that contained in paragraphs 16 and 17. I have said (1330.), "what then is to separate the principle of these two extremes, perfect conduction and perfect insulation, from each other, since the moment we leave in the smallest degree perfection at either extremity we involve the element of perfection at the opposite end?" and upon this you say, might not this query be made with as much reason in the case of motion and rest?—and in any case of the intermixture of opposite qualities, may it not be said, the moment we leave the element of perfection at one end, we involve the element of perfection at the opposite?—may it not be said of light and darkness, or of opakeness and translucency? and so forth.

xxxiii. I admit that these questions are very properly put;

not that I go to the full extent of them all, as for instance that of motion and rest; but I do not perceive their bearing upon the question, of whether conduction and insulation are different properties, dependent upon two different modes of action of the particles of the substances respectively possessing these actions, or whether they are only differences in *degree* of one and the same mode of action? In this question, however, lies the whole gist of the matter. To explain my views, I will put a case or two. In former times a principle or force of levity was admitted, as well as of gravity, and certain variations in the weights of bodies were supposed to be caused by different combinations of substances possessing these two principles. In later times, the levity principle has been discarded; and though we still have imponderable substances, yet the phænomena causing weight have been accounted for by one force or principle only, that of gravity; the difference in the gravitation of different bodies being considered due to differences in *degree* of this *one force* resident in them all. Now no one can for a moment suppose that it is the same thing philosophically to assume either the two forces or the one force for the explanation of the phænomena in question.

xxxiv. Again, at one time there was a distinction taken between the principle of heat and that of cold: at present that theory is done away with, and the phænomena of heat and cold are referred to the same class, (as I refer those of insulation and conduction to one class,) and to the influence of different degrees of the same power. But no one can say that the two theories, namely, that including but one positive principle, and that including two, are alike.

xxxv. Again, there is the theory of one electric fluid and also that of two. One explains by the difference in degree or quantity of one fluid, what the other attributes to a variation in the quantity and relation of two fluids. Both cannot be true. That they have nearly equal hold of our assent, is only a proof of our ignorance: and it is certain whichever is the false theory, is at present holding the minds of its supporters in bondage, and is greatly retarding the progress of science.

xxxvi. I think it therefore important, if we can, to ascertain whether insulation and conduction are cases of the same class, just as it is important to know that hot and cold are phænomena of the same kind. As it is of consequence to show that smoke ascends and a stone descends in obedience to one property of matter, so I think it is of consequence to show that one body insulates and another conducts only in consequence of a difference in degree of one common property

which they both possess; and that in both cases the effects are consistent with my theory of induction.

xxxvii. I now come to what may be considered as queries in your letter which I ought to answer. Paragraph 8 contains one. As I concede that particles on opposite sides of a vacuum may perhaps act on each other, you ask, "wherefore is the received theory of the mode in which the excited surface of a Leyden jar induces in the opposite surface a contrary state, objectionable?" My reasons for thinking the excited surface does not directly induce upon the opposite surface, &c., is, first, my belief that the glass consists of particles conductive in themselves, but insulated as respects each other (xvii.); and next, that in the arrangement given iv., ix., or x., A does not induce directly on C, but through the intermediate masses or particles of conducting matter.

xxxviii. In the next paragraph, the question is rather implied than asked—what do I mean by polarity? I had hoped that the paragraphs 1669. 1670. 1671. 1672. 1679. 1686. 1687. 1688. 1699. 1700. 1701. 1702. 1703. 1704. in the *Researches*, would have been sufficient to convey my meaning, and I am inclined to think you had not perhaps seen them when your letter was written. They, and the observations already made (v., xxvi.), with the case given (iv., v.), will, I think, be sufficient as my answer. The sense of the word *polarity* is so diverse when applied to light, to a crystal, to a magnet, to the voltaic battery, and so different in all these cases to that of the word when applied to the state of a conductor under induction (v.), that I thought it safer to use the phrase "species of polarity," than any other, which being more expressive would pledge me further than I wished.

xxxix. Paragraph 11 involves a mistake of views. I do not consider bodies which are charged by friction or otherwise, as polarized, or as having their particles polarized (iii., iv., xxvii.). This paragraph and the next do not require, therefore, any further remark, especially after what I have said of polarity above (xxxviii.).

xl. And now, my dear sir, I think I ought to draw my reply to an end. The paragraphs which remain unanswered refer, I think, only to differences of opinion, or else, not even to differences, but opinions regarding which I have not ventured to judge. These opinions I esteem as of the utmost importance; but that is a reason which makes me the rather desirous to decline entering upon the reconsideration, inasmuch as on many of their connected points I have formed no decided notion, but am constrained by ignorance and the

contrast of facts to hold my judgement as yet in suspense. It is, indeed, to me an annoying matter to find how many subjects there are in electrical science, on which, if I were asked for an opinion, I should have to say, I cannot tell,—I do not know; but, on the other hand, it is encouraging to think that these are they which if pursued industriously, experimentally, and thoughtfully, will lead to new discoveries. Such a subject, for instance, occurs in the currents produced by dynamic induction, which you say it will be admitted do not require for their production intervening ponderable atoms. For my own part, I more than half incline to think they do require these intervening particles, that is, where any particles intervene (1729. 1733. 1738.). But on this question, as on many others, I have not yet made up my mind. Allow me, therefore, here to conclude my letter; and believe me to be, with the highest esteem,

My dear Sir,

Your obliged and faithful Servant,

Royal Institution, April 18, 1840.

M. FARADAY.

X. Notices respecting New Books.

Report on the Progress of Vegetable Physiology during the Year 1837.

By F. J. F. MEYEN, M.D., Professor of Botany in the University of Berlin. Translated from the German, by WILLIAM FRANCIS, A.L.S. London, 1839. 8vo, pp. 158.

OUR readers will doubtless remember the valuable Report on the Progress of Vegetable Physiology for the year 1836, which appeared in our pages about two years since*. The high position occupied by Professor Meyen in this department of science, and the vast increase which is constantly being made in the amount of our knowledge of it, by the labours of the industrious physiologists of Germany, combine to give these reports a peculiar value. A great part of the information contained in them would not have found its way to this country in any other shape; and it is much more agreeable to obtain it in the condensed form it assumes after being submitted to the Professor's *compressorium*,—which squeezes away the lighter fluid with which it is diluted, and retains the solid matter,—than in its original state. The Report at present before us is equally full of valuable information with the former one; and, when its much greater extent is considered, its importance as a contribution to scientific literature will be apparent. The rapid advance of discovery in this most interesting science will give, we are assured, a progressively increasing value to these reports: and when the utility of a well-executed translation is considered, especially from a language which needs long study and familiarity to give a certainty of the author's meaning being understood, we

* Lond. and Edinb. Phil. Mag., vol. ix. p. 381, *et seq.*

trust that Mr. Francis may receive sufficient encouragement to induce him to persevere in a labour which can, at best, be but scantily remunerated. At present, we regret to learn, he has not cleared the expenses of publication. We would urge those of our readers, therefore, who are interested in the promotion of this department of botanical science (as all who call themselves botanists ought to be), to do what in them lies for the continued success of this undertaking, calculated as it is in the highest degree for their benefit.

XI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xvi. p. 148.]

Nov. 6, **A** PAPER was afterwards read, "On the relative ages of 1839. the tertiary and post-tertiary deposits of the Basin of the Clyde," by James Smith, Esq., of Jordan Hill, F.G.S.; of which an abstract will be found in No. 65 of the Society's Proceedings.

A paper was last read, "On the foul air in the chalk and strata above the chalk near London," by James Mitchell, LL.D., F.G.S.

In the chalk, the most abundant deleterious gas is the carbonic acid, but it has been found to exist in greater quantity in the lower than in the upper portion of the formation, and in that division to be unequally distributed. In sinking wells, it has been noticed to issue with force from one stratum, whilst none has been perceived to be given out from the beds immediately above and below it. Dr. Mitchell mentions fatal effects due to its occurrence in a well near the race-course at Epsom, where it was met with at the depth of 200 feet; and in Norbury Park, near Dorking, at the depth of 400 feet. On Bexley Heath, after sinking through 140 feet of gravel and sand and 30 feet of chalk, it rushed out and extinguished the candles of the workmen; and in making a well in Long Lane, Bexley Heath, after penetrating 124 feet of overlying deposits, and then 90 feet of chalk, considerable inconvenience was felt from it, but 6 feet lower no gas was emitted.

In chalk, sulphuretted hydrogen gas is also occasionally met with, and is supposed to be generated from the decomposition of water and iron pyrites.

In districts in which the chalk is covered with sand and London clay, carburetted hydrogen gas is sometimes emitted, but more frequently sulphuretted hydrogen gas.

Carburetted hydrogen has seldom inflamed in wells, but in making the Thames tunnel it has sometimes issued in such abundance as to explode by the lights and scorch the workmen.

Sulphuretted hydrogen gas is more abundant, and it has been observed almost always to proceed from a coarse black sand charged with oxide of iron, whether the bed be above the blue clay, within it or below it. It has streamed out with great violence in the Thames tunnel, but has in no instance produced fatal effects. At Ash, 3 miles

from Farnham, a well was dug in sand to the depth of 36 feet, and one of the workmen on descending into it was instantly suffocated. Fatal effects have also resulted from the accumulation of this gas in wells in Maiden-lane, Battle-bridge, and at Applebury-street, near Cheshunt. This gas is much increased, after long-continued rain, in consequence of the swelling of the clay driving it out of the interstices; and it is diminished after a long drought. The prevalence of a north-east wind has been noticed by well-diggers to diminish the quantity of the gas, but the effect is ascribed by Dr. Mitchell to the dry weather which usually accompanies the wind from that quarter. The author also suggests, that if wells are to be dug in dangerous districts, they should be undertaken when there is least water in the ground, or from the beginning of July to October.

The noxious gas in the Weald of Kent and Sussex is stated to be sulphuretted hydrogen.

November 20, 1839.—A paper "On the origin of the vegetation of our Coal-fields and Wealdens," by J. T. Barber Beaumont, Esq., F.G.S., was next read.

An examination of the fossil trees discovered on the line of the Manchester and Bolton Railway* has confirmed Mr. Beaumont in the opinion, that in no instance has the vegetation of the coal-fields arisen from drifted trees sunk to the bottom of mighty rivers and estuaries, but that it grew where it is found; and he is further of opinion, that the districts composing our present coal-fields were originally islands.

The principal objections of the author to the theory of the transportation of the fossil vegetation are the following:

1. The existence of a mighty river or estuary at the time of the deposition of the coal-measures, would require the existence of a vast continent of which there are no traces.

2. The coal strata near Newcastle are 380 yards in thickness, and consequently, the lowest strata must have been deposited at the bottom of a river or estuary, exceeding in depth, six times the mean depth of the German Ocean.

3. A continent producing such a river, it is reasonable to expect, would have left an abundance of fossils on its surface, as well as at the bottom of its great river; but all the land for hundreds of leagues around the coal and wealden formation swarms with the remains of marine animals, and is clearly an ancient sea bed.

4. In the coal-measures not a bone of a land quadruped is to be found, or a large timber tree, with the exception of a few Coniferae.

5. In order that the vegetation should have sunk to the bottom of a deep river, it must first have decayed; but the plants preserved in the beds associated with the coal, present a freshness and perfection incompatible with such a condition.

6. Drifted trees are stopped in deltas only from the shallowness of the water being insufficient to float them on; we know of no deposits of trees in deep water.

* See the Abstract of Mr. Hawkshaw's Paper, *Proceedings*, vol. iii., p. 139; [or *L. & E. Phil. Mag.* vol. xv. p. 539.]

The author then offers the following theory as affording a preferable explanation of the origin of coal-fields and Wealden formations.

He conceives they were originally swampy islands, formed out of disjointed fragments which resulted from the first elevation of the rocks; that on these islands grew a luxuriant vegetation consisting of Ferns, Calamites, coniferous trees, &c. which, decaying and regenerating, accumulated in the manner of peat bogs; that the islands, by the settling down of the disturbed crust of the earth, were depressed beneath the surface of the sea, and covered over with drifted sand, clay, and shells, till they were again, by this process, converted into dry land, and clothed with another vegetation; and he is of opinion, that the operation was repeated as many times as there are alternations of coal and sedimentary strata.

A notice on "The Fossil Fishes of the Yorkshire and Lancashire Coal-fields," by W. C. Williamson, Esq., was then read; of which an abstract is given in No. 65 of the Proceedings.

A paper was last read, entitled, "A brief notice of the Geology around the shores of Waterford Haven," by Major Austin.

In this memoir, the author describes topographically the geology of Waterford Haven, commencing at Bag and Bun Head, near its eastern entrance, and proceeding around its shores terminates his account at Ballymacaw. The formations of which the district consists are, 1. limestone; 2. a conglomerate; 3. clay-slate; 4. various trap-rocks; and 5. alluvium.

1. The limestone constitutes the promontory forming the eastern boundary of the Haven from Hook Point to the parallel of Sand Eel Bay, a distance of about $3\frac{1}{2}$ miles. Some of the beds are of a dark colour, and are so fissile as to be used for roofing-slate; they are also highly sonorous when struck with a hammer, and are in consequence locally called the "Black Bell." In the lower part of the formation, in Bryce's Bay, the limestone is yellow. Fossils, including spined *Productæ* and *Crinoideæ*, compose the greater part of some of the strata. Near the conglomerate the limestone alternates with thin layers of slate, and at the immediate junction is a stratum of fine red sand containing impressions of numerous fossils. The strata, where undisturbed, dip to the S.S.W. at an angle of about 22° , but they are sometimes considerably contorted.

2. The conglomerate composes a band about half a mile broad, stretching across the promontory from Sand Eel Bay to Herrylock, and is of a deep-chocolate colour. It constitutes Broom Hill, where it assumes partly the character of a compact grit, and partly that of a coarse compound of fragments of schist, quartz, and other pebbles imbedded in a chocolate-coloured cement; the quartz pebbles being traversed by numerous fissures. Other patches of conglomerate occur between King's Bay and Buttermilk Castle, and are said to overlay the edges of highly-inclined strata of clay-slate. It forms also the hills north of the Suire in the county of Kilkenny, where it is worked for millstones, and it composes, with a few interruptions, the whole of the west side of the Haven.

3. The clay-slate is extensively developed on the east side of the

Haven from Temple Bay to Great Island at the confluence of the Suire with the Nore and the Barrow, forming a series of cliffs, in which the strata are excessively contorted. It also forms the cliffs from the west shore of the Nore opposite Little Island and the right bank of the Suire; a small patch of it occurs likewise south of Passage. The contortions in the cliffs on the east side of the Haven, Major Austin ascribes to lateral pressure, and he states that the ridges of clay-slate range at right angles to the band of conglomerate which traverses the promontory at Sand Eel Bay. These contortions are more especially marked and numerous between Dollar Bay and Bluff Head, ranging from the top to the bottom of cliffs 250 feet in height. At the south point of Booley Bay the slate affords beautiful examples of ripple marks. It is stated to be perforated along the whole line of coast, by hollows varying in size from a pin's point to five inches in length, and to the height of 20 feet above high-water mark. On the hill above the Roman Catholic chapel, north of Duncannon sands, the slate has a prismatic structure. It is stated to have been altered by igneous operations on the west side of Great Island and in Little Island. Near Ballyhack the cleavage is diagonal to the plane of stratification.

4. Trap forms at Herrylock a mass protruding from the beach; also the narrow point on which Duncannon Castle stands, where it is stated to rest on the tilted edges of clay-slate, and to have altered that rock into an indurated, stratified, yellow stone. A basaltic dyke occurs opposite Great Island on the Kilkenny side of the Nore. It rises abruptly to the height of 100 feet, and from its white colour, due to the decomposition of the felspar, is called the White Horse. A considerable mass of trap is also at the Orphan School House; and other masses at Cheek Point in the bend of the Haven to the southward, and at Newtown on the western shore of the Haven.

5. Alluvium. Modern accumulations occur in Dollar Bay; Aldridge Bay; to the north of Bluff Head, where a layer of slate shingle is converted into a coarse conglomerate by carbonate of lime; between Bluff Head and Duncannon, constituting the Duncannon sands, and extending inland up a small valley, in which is a peat bed; the narrow valley at King's Bay or Arthurstown; to the south of Dunbrody Abbey; and in Woodstown valley, on the west side of the Haven. Major Austin states, that greenstone boulders occur in all parts of Wexford, but that no rock *in situ* of character similar to them is to be found in the county.

December 4, 1839.—A paper was first read, entitled "A Description of the Soft Parts and of the shape of the Hind Fin of the Ichthyosaurus, as when recent," by Richard Owen, Esq., F.R.S., F.G.S.

The osseous frame-work of the fin of the Ichthyosaurus, Mr. Owen observes, having alone been the subject of direct examination, the exact shape and the nature of the soft parts had been matters of conjecture. A very striking deviation from the reptilian and mammalian types had, indeed, been recognised, and resemblance also to the fins of fishes had been admitted in the digits of the fin exceeding five, in their being sometimes bifurcated, and in consisting of an ex-

traordinary number of ossicles; yet owing to the form of the digital ossicles, their breadth and flatness, and their large size, as compared with the joints of the fin-rays of fishes, it had been generally supposed that the locomotive organs of the *Ichthyosaurus* were enveloped, while living, in a smooth integument, which like that of the turtle and porpoise, had no other support than was afforded by the bones and ligaments within.

Sir Philip Grey Egerton in a recent examination of *Ichthyosaurian* remains in the possession of Mr. Lee of Barrow-on-Soar, detected, with the penetration which has enabled him to bring to light many other obscure points in the structure of the *Ichthyosaurus*, traces of the soft parts of the fin in a slab of lias containing a mutilated paddle; and having submitted the specimen to the examination of Mr. Owen, a detailed account of its character forms the subject of this memoir.

Mr. Owen considers the specimen to be a posterior fin of the *Ichthyosaurus communis*. It presents impressions and fractured portions of six digits, with the impression,—and a thin layer, most distinctly preserved,—of the dark carbonized integument of the terminal half of the fin, the contour of which is thus most beautifully defined.

The anterior margin is formed by a smooth unbroken well-marked line, apparently a duplication of the integument; but the whole of the posterior margin exhibits the remains and impressions of a series of rays by which the fold of the integument was supported. Immediately posterior to the digital ossicles, is a band of carbonaceous matter of a distinctly fibrous structure, varying from two to four lines in breadth, and extending in an obtusely-pointed form for an inch and a half beyond the digital ossicles. This band Mr. Owen believes to be the remains of the dense ligamentous matter which immediately invested the bones of the paddle, and connected them with the enveloping skin. The rays, above mentioned, are continued from the posterior edge of this carbonized ligamentous matter, in which their bases appear to have been implanted, to the edge of the tegumentary impression; the upper rays being directed transversely, but the others gradually lying more in the direction of the axis of the fin, as they approach its termination. The most interesting feature in these rays, Mr. Owen says, is their bifurcating as they approach the edge of the fin.

From the rarity of their preservation, their appearance and co-existence in the present instance with remains of the integument, he states, it is evident they were not osseous, but probably either cartilaginous, or of that albuminous horn-like tissue, of which the marginal rays consist in the fins of the sharks and other plagiosomous fishes. Besides the impression of the posterior marginal rays, the specimen presents a series of fine, raised, transverse lines, which cross the whole fin, and probably indicate a division of the rigid integument into scutiform compartments, analogous to those on the paddle of the Turtle and webbed foot of the Crocodile; but they differ in the absence of subdivision by secondary longitudinal

impressions. The structure of the integument of the fin agrees, therefore, with the known reptilian characters of the skeleton of the Ichthyosaurus; and, as the skin with its appendages gives a character to the great primary groups of vertebrata, it might be expected that the skin of the Ichthyosaurus would exhibit some of the characters of the integument of existing reptiles.

In conclusion, Mr. Owen remarks, that the other new facts presented by the specimen, accord with the indications of the natural affinities of the Ichthyosauri afforded by their less perishable remains; and that all the deviations from the reptilian structure of the skeleton tend to the type of fishes and not to that of cetaceous remains.

A paper was afterwards read on as much of the great graywacke system as is comprised in the group of West Somerset, Devon, and Cornwall, by the Rev. D. Williams, F.G.S.

This communication is supplementary to one read in April 1839*, and contains the results of the author's last investigation into the structure of the country. Before he details his present views, he corrects what he conceives was an error in his former paper, and states, he is now convinced that the slates and limestones of South Devon and Cornwall are not a prolongation of the trilobite slates (No. 7)† of Exmoor, but a distinct formation superior to the floriferous grits or culm measures (No. 9,) of central Devonshire, and consequently the newest deposits of the country. For this formation he proposes the name of Killas or Cornish, and he considers it as No. 10 in the ascending series. According to the new arrangement therefore, in proceeding from those parts of West Somerset bordering the Bristol Channel to the south of Devon and Cornwall, he conceives a regular ascending series is passed over, the central portion of which he is of opinion, consists of the floriferous grit or culm measures (9). He places the whole series also in the graywacke system.

Mr. Williams's reasons for this arrangement are drawn from observations made at several localities in South Devon.

At Doddiscombe Leigh, about four miles to the north of Chudleigh, the Posidonia limestone, a member of No. 8, the Coddon Hill grit, underlies, Mr. Williams says, a long series of alternations, exposed on the turnpike road towards Chudleigh, and consisting of high hills of Coddon and floriferous grits with intercalated killas, the whole dipping beneath the coral limestones of Chudleigh, which pass under the ridge of Ugbrook considered by Mr. Williams to be composed of floriferous grits. On closely examining the limestones about Chudleigh, the author discovered minor alternations, which

* See Proceedings, vol. iii., p. 115, [or L. & E. Phil. Mag., vol. xv., p. 396.]

† The following is the descending arrangement given in Mr. Williams's former paper: 9, floriferous slates and sandstones (*culm measures*); 8, Coddon Hill grits; 7, trilobite slates; 6, Wollacomb sandstones; 5, Morte slates; 4, Trentishoe slates; 3, calcareous slates of Linton; 2, Foreland or Dunkerry sandstone; 1, Cannington Park limestone.

exhibited in juxtaposition the several varieties of Cornish killas, pale green, light blue, and purple volcanic ash and Coddon Hill grits (the two latter containing plants), interstratified beyond any doubt among the coral limestones, and these limestones inclosing carbonaceous beds, underlaid and overlaid by thick accumulations of Coddon Hill and floriferous grits. On the road by Grayleigh, and descending further on to Waddon Burton, Mr. Williams has traced these alternations foot by foot, and he says the sequents are so manifestly palpable in their interchanges, great and small, that nothing is left for inference or conjecture.

The inseparable connexion of the coral limestone and Cornish killas with the floriferous series, he states, is also most plainly exhibited to the N.E. of Kingsteignton, at Ashburton, Newton Bushel, Abbots, and King's Kerswell, Marlton, Berry Pomeroy, Higher and Lower Yalberton, and thence to Brixham and Berry Head. At Meadfort sands, on the south, the author says, there is a strange intermixture, yet in regular stratified order, of Cornish clay slate, and buff-coloured, finely arenaceous beds inclosing shells and corals, with true floriferous sandstones containing plants, also with culmy slates, and strata of volcanic ash and coral limestones, forming an anticlinal axis, ranging north and south, and throwing off the great mass of the Torquay limestones to the east and west.

To the east of Dartmoor, Mr. Williams has little doubt the same alternations on a great and small scale occur, as far as the parallel of Torquay, immediately to the south of which line, but west of the bay, are said to be lofty conspicuous hills of the floriferous strata (No. 9), which he conceives "may be abruptions"; but their bases are commonly concealed by a thick mantle of new red sandstone. To the west of Dartmoor, independent of what he believes to be an axis of No 8, and forming the southern margin of the culm-trough, Mr. Williams has been led to conclude, that the floriferous beds, by other arcs of undulation, have broken through the thinner northern border of the killas, throwing it off on each shoulder and intercepting it in troughs. Instances of this, he says, are sufficiently apparent on each side of a line drawn from Greston Bridge on the Tamar, S.E. of Launceston, to Heath Field, north of Tavistock; the high central ridge consisting of floriferous and Coddon grits, flanked on the north by undoubted killas and volcanic ash and breccia. To the N.W., W.S.W., and S. of Greston Bridge, the floriferous series is stated to be exposed in great force, extending almost uninterruptedly in a straight line into the great body of the culm-field; but to the west it is said to be generally concealed by killas, though a continuous contracted range branches off to the westward by Lezant, Lawanick-down, and Alternau; and it appears to be depressed under, or to pass into the killas and speckled slates of Davidstow, N.E. of Camelford.

A small area of an inverted arc of undulation is exposed in and around Tavistock; and the floriferous grit, principally identified by its associated slates, is stated to underlie the high killas range of Whitcombe Down on the south; and on the north to be laid open in

the road to Launceston, similarly associated, with the additional accompaniment of intercalated killas and Coddon Hill grit dipping beneath the volcanic ash of Marly Mead. Mr. Williams also mentions a shaft sunk in a copper lode close by the turnpike gate on the road from Tavistock to Callington, as another instance of floriferous grit, or a rock exactly the same as that which contains the copper lode of Wheal Friendship, underlying pale green slate or killas: and he adds, there can be no doubt that Wheal Friendship mine is in No. 9 or the floriferous series. The next instance mentioned in the paper, is at Linkinghorn and South Hill, north of Callington, where there is stated to be an entire suite of alternations of killas, Coddon grits, and floriferous grits with plants, all dipping south. Again, near Pillaton, between Callington and Saltash, there are said to be countless minor alternations of floriferous and Coddon grits, each thinning out and interlocking like the teeth of a trap. Another instance of the killas overlying the floriferous grit, is said to occur in a hill at Penter's Cross on the road from Callington to Saltash, where a cutting exhibits in the lower part, a series of alternations of floriferous sandstone, and culmy slates which in ascending disappear, and in their place a delicate pale green killas alternates with the sandstone beds, while at the summit the sandstones disappear and are replaced altogether by killas. A fault traverses the hill, and to the south of it only killas occurs.

The general results of his observations, Mr. Williams says, show, that in the ascending order, from Cannington Park and the Quantocks in Somersetshire, to the Land's End, there is a group constituted of ten strikingly simple, consecutive series, severally varying in their mineral and zoological character; that as respects the limestone suites, the thin and spare dimensions of such as occur in the Trilobite slates of Exmoor (No. 7) render them too insignificant to be noticed; that the *Posidonia* limestones, which by mineral gradation succeed and conformably overlie the latter, are elliptically included in the Coddon Hill grits (No. 8), and together distinctly underlie and constitute the base of the great floriferous series (No. 9): that higher up in No. 9 is an extended horizon, which separates the series into an upper and a lower, containing the Bampton, Hockworthy, Holcomb Rogus, and Hastleigh limestones, with red and black slates on the north; the Petherwen and Landlake slates and limestones on the south; and the entire suite of coral limestones to the east of Dartmoor, extending from Chudleigh to Berry Head and Brixham: that the Plymouth limestones included in the killas (No. 10) are higher in the group than those just mentioned, and are introduced first at Millaton, about a mile and a half west of St. Germans.

Mr. Williams considers the slate or killas series of S. Devon to be distinguished from the slates of Exmoor by a peculiar extraneous cleavage. In Exmoor the cleavage, he says, is at all angles, from less than 10° to the vertical, its planes having a direction of about east and west; whilst the cleavage lines of the killas either coincide with the magnetic or true meridian, or depart from it to the east or

west only a few degrees, and the inclination approaches the vertical with a strike N. and S., or nearly perpendicular to that of Exmoor.

What, says Mr. Williams, are the results, if the question be tested by the assumed law, that strata may be identified by their organic remains? If the *Posidonix* and *Goniatites* of the lenticular, black limestones, never exceeding thirty-five feet in thickness, be appealed to, to identify them with the mountain limestone, the weight of organic remains opposed to them in the Launceston and Petherwen fossils, the corals and other organic remains of South Devon belonging to the floriferous series, reduces the evidence to dust. If mineral characters be appealed to, he says, they fail altogether.

In conclusion, Mr. Williams remarks, that in this supplement, he has endeavoured faithfully to transfer the simple truths of nature to his pages, without reference to the theories of others. He would, however, remind geologists that the proposed law of Mr. W. Smith is no law, if it do not imply a final and universal extinction of species. This being his own view, Mr. Williams says, he could not admit that the *Goniatites* and *Posidonix* of Devonshire were first introduced and became extinct with the mountain limestone, being justified by the fact of superposition, and more reasonable analogy, in concluding that these genera existed elsewhere in congenial conditions during the entire period of the deposition of the Trilobite slates, when that formation ceasing in Devonshire, the ova of the creatures or the creatures themselves were transported to a region favourable to their existence, and were continued during epochs of duration up to the period of the mountain limestone, and probably beyond it, if they be not now in existence. They appear, in his opinion, like the corals of Devon, to have been subject to repeated mineral accidents, and to have been locally destroyed in groups, not universally effaced in species. To guard himself, however, from misconstruction, Mr. Williams adds, he believes entirely in the extinction of genera and species; but at very distinct epochs, and in far thicker and more extended groups of strata than is imagined; and that consequently the identification of strata must be regulated by a per-centage test similar to that applied by Mr. Lyell to the tertiary series. Lastly, he protests against the determination of the age of the Devonshire formations by reference to the structure of a foreign district.

PROCEEDINGS AT THE FRIDAY-EVENING MEETINGS OF THE
ROYAL INSTITUTION.

- May 1.—Mr. Griffiths on the sources and uses of sulphuric acid.
- May 8.—Mr. Faraday on the origin of electricity in the Voltaic pile.
- May 15.—Mr. Macilwain on respiration and its relation to animal temperature.
- May 22.—Mr. Brande on white lead.
- May 29.—Mr. Brockedon on some new applications of caoutchouc.
- June 5.—Rev. Dr. Scoresby on magnetism.
- June 12.—Mr. Carpmæl on the manufacture of wire cards.

XII. *Intelligence and Miscellaneous Articles.*

LETTER TO PROF. LIEBIG ON THE THEORY OF SUBSTITUTIONS*.

SIR,

I HASTEN to communicate to you one of the most brilliant facts of organic chemistry. I have verified the theory of substitutions in an extremely remarkable and perfectly unexpected manner. It is only from the present time that we shall be able to appreciate the high value of this ingenious theory, and to foresee the immense discoveries it promises to realize. The discovery of chloracetic acid, and the constancy of the types in the chlorinated (*chlorés*) compounds derived from æther and the chloride of æthyle, have led me to experiments which I will now describe. I passed a current of chlorine through a solution of the acetate of manganese under the direct influence of solar light. After twenty-four hours I found in the liquid a superb crystallization of a yellow violet salt. The solution contained nothing further than this salt and hydrochloric acid. I analysed this salt: it was the chloracetate of the protoxide of manganese. Nothing extraordinary as yet; a simple substitution of the hydrogen of the acetic acid by an equal number of equivalents of chlorine, already known from the beautiful experiments on chloracetic acid. This salt, heated to 110° in a current of dry chlorine, was converted with disengagement of oxygen into a new compound of a gold yellow colour, the analysis of which had for its composition to the formula $\text{Mn Cl}_2 + \text{C}_4 \text{Cl}_6 \text{O}_3$. There was therefore a substitution of the oxygen of the base by chlorine, similar to what has been observed in a multitude of cases. The new substance dissolved in quite pure chloral with the aid of heat. I employed this liquid, unalterable by chlorine, in order to continue the treatment by means of this agent. I passed dry chlorine during four days, keeping the liquid constantly near its boiling point. During this time a white substance was constantly deposited, which, when attentively examined, proved to be the chloride of protochloride (?) of manganese. I cooled the liquor when all precipitation had ceased, and obtained a third body in small needles, silky and of greenish yellow colour; it was $\text{C}_4 \text{Cl}_{10} \text{O}_3$, or in other terms it was the acetate of manganese, in which all the hydrogen and the oxide of manganese had been replaced by chlorine. Its formula should be written $\text{Cl}_2 \text{Cl}_2 + \text{C}_4 \text{Cl}_6 \text{O}_3$. There were therefore six atoms of chlorine in the acid, the four other atoms representing the oxide of manganese. Just as hydrogen, so also manganese and oxygen may be replaced by chlorine, and nothing surprising will be found in this substitution. But this was not the end of this remarkable series of substitutions. On letting a new chlorine act on a solution of this substance in water, there was a disengagement of carbonic acid; and on cooling the liquid to $+ 2^{\circ}$, a yellowish mass, formed of small laminæ, was deposited, very much resembling the hydrate of

* From Liebig's *Journal der Chemie und Pharmacie*, vol. xxxiii. part 3.

chlorine, and indeed it consisted of nothing further than chlorine and water. But in taking the density of its vapour I found that it was formed of twenty-four atoms of chlorine and of one atom of water. Here then is the most perfect substitution of all the elements of the acetate of manganese. The formula of the substance should be expressed by $\text{Cl } 2 \text{ Cl } 2 + \text{Cl } 8 \text{ Cl } 6 \text{ Cl } 6 + \text{Cl } 9$. Although I am aware that in the bleaching action of chlorine there is a substitution of the hydrogen by the chlorine, and that the stuffs which are at present bleached in England, according to the law of substitutions, preserve their types*, I nevertheless believe that the substitution of carbon by chlorine, atom for atom, is a discovery belonging to me. I trust you will take a memorandum of this note in your valuable Journal, and believe, &c.,

SCHWINDER†.

ON THE FORMATION OF LAMPIC ACID.

Lampic acid, according to the experiments of Stas and Martius, is a mixture of formic and aldehyd acids. Mr. R. F. Marchand states that these acids vary in their respective proportions in the combination according to the temperature of the platina wire generating the acid, and that they may be obtained in a constant proportion, which may be nearly accurately determined when the experiment is made with alcohol or æther. When alcohol or æther is dropped upon a red-hot platina dish, the peculiar phænomenon is instantly produced; the liquid runs about the red-hot metallic surface without quickly evaporating, and forms the known figures which have been described by Böttcher. The vapour may be collected by putting a tubulated glass retort with the bottom off over the platina dish. By pouring fresh liquid through the tube a considerable quantity may be obtained in a short time. Upon examination it is easily ascertained that it is no longer alcohol, but possesses all the properties of lampic acid. The composition, or rather the mixture of the two ingredients, varies according to the temperature of the platina dish, in the same manner as that produced by platina wire. In order to ascertain whether the effect was peculiar to platina, Mr. Le Marchand tried glass, porcelain, *polished* copper and iron dishes; these also produced the same effects. They must, however, be polished and not present any rough surfaces; in the latter case the peculiar effect was not produced: it is also not produced when sand or glass is strewed over the platina dish.

The temperature to which the alcohol rises varies (as is the case with water) according to the heat of the dish and to the size of the drop. With water Mr. Le Marchand found it very accurately beginning at 180° , increasing to 204° , and then to 212° Fahr.: arrived at this point, the experiment is over, by the boiling of the water. The

* I have just learnt that there are already in the shops of London stuffs in spun chlorine (*étouffes en chlore filé*), much in request, and preferred to everything for night-caps, drawers, &c.

† [*Schwinder* we presume means Hoaxer.]—EDIT.]

same takes place with regard to alcohol; the temperature, beginning at 140° , rises at last to the boiling point of the liquid. When this takes place, the greater part of the alcohol burns; but before the burning commences, light blue flames are visible, which Dœbereiner also observed upon the vaporization of æther dropped on hot platina, Mr. R. F. Marchand wishes to guard against the inference that might be drawn from this experiment, that he considered that the liquids used were decomposed. The formation of lampic acid is based upon the fact, that the undecomposed vapour of alcohol passing within a certain distance, immediately over the hot metallic surface, is oxidated, which is also the case with platina wire. If the vapour were in actual contact with the metallic surface it would inflame.

Both experiments are a confirmation of Buff's explanation of the repulsion in Poggendorff's Annals, xxi., which is so full and satisfactory that it is unnecessary to look for any further elucidation.—*Journal für Praktische Chemie*, 1840. No. 1.

ANALYSIS OF THE ASHES OF THE *Salsola Tragus*.

M. Guibourt has analysed the ashes of this plant from the neighbourhood of Cherbourg. He found that it consisted, independently of some silica, of

Carbonate of potash	29.04
Chloride of potassium	17.89
Sulphate of potash	4.93
Carbonate of lime	40.26
Phosphate of lime and oxide of iron	7.88

100.

M. Guibourt observes, that it is curious that the salts of the ashes should contain potash as the alkali, and that it constitutes an exception to a maritime plant containing no soda.—*Journ. de Chim. Med.* Mars, 1840.

UNCOMBINED HYPOSULPHUROUS ACID.

M. Langlois, professor of chemistry at Strasburg, has succeeded in isolating hyposulphurous acid; he obtained it perfectly pure by decomposing hyposulphate of potash, with oxichloric acid, which forms an insoluble salt with potash.

The acid thus obtained is liquid, colourless, and of a slightly syrupy consistence. A period arrives when its density cannot be increased without partially decomposing it. Its taste is strongly acid and bitter; it does not appear to be very caustic. When exposed to the air, it attracts moisture, when heated in a glass tube to 176° Fahr., and there are produced a deposit of sulphur and sulphurous acid gas. It does not render the solutions of the salts of lime and strontia turbid. It produces no effect upon the solutions of the salts of iron, zinc, or copper; but with the salts of lead it yields a white precipitate which becomes black when heated. In the solution of nitrate of silver it forms at first a yellowish preci-

pitae, which soon becomes black, and sulphuret and sulphate of silver are produced. The salts of mercury and platina are precipitated black by this acid; and it will be observed that it acts on the different salts in the same way as hyposulphate of potash.

Nitric acid reacts instantaneously on concentrated hyposulphurous acid, nitric oxide is evolved, sulphur is deposited and the solution contains sulphuric acid. The action of chloric acid is not less remarkable than that of nitric acid; the decomposition of both acids occurs instantaneously, with tumultuous action; sulphur and chlorine evidently appear, and reagents show the presence of sulphuric acid in the solution: the phenomena are similar to those observed when chloric acid is dropped into alcohol or æther. In this latter case as in those also happens inflammation of the excess of the combustible body.—*L'Institut*, No. 327.

ON THE PRESENCE OF IODINE IN COD OIL. BY R. F. MARCHAND.

The different statements of chemists with regard to the presence of iodine in cod oil (Leberthran) appear to have arisen from the circumstance of those who have not found it having examined impure oil. Cod oil was examined by a chemist of this place which had been received direct from Bergen, without his having succeeded in detecting any iodine in it.

As in all probability its energetic action depends upon the presence of iodine, that statement might lead the medical professor to reject Bergen cod oil. I have also received cod oil from the same source which was certainly pure. I examined it according to Gmelin's method (*Ann. der Pharm.*, B. xxxi. p. 523.) which is both a simple and sure method. This indicated the presence of iodine in a manner about which there could not be the least doubt. Twenty grammes was about the quantity acted upon.—*Journal für Praktische Chemie*. 1840. No. 4.

MR. M'CORD'S OBSERVATIONS ON THE SOLAR AND TERRESTRIAL RADIATION MADE AT MONTREAL.

A Meteorological Register for 1838 was some time since kindly communicated to us by Dr. Daubeny, kept at Montreal, Lower Canada, in lat. 45° 50' N., lon. 73° 22' W., by J. S. M'Cord, Esq., Corresponding Secretary to the Natural History Society, and Member of the Literary and Historical Society, Quebec. It is an abstract of observations of the Barometer, Thermometer, Rain Gauge, Snow Gauge, Actinometer, and Register Thermometer; all made by the first British artists, and carefully compared with standard instruments.

The mean pressure of the year, corrected and reduced to 32° Fahr. is 29·884 in.; the mean temperature, (mean of maxima and minima by Register Thermometers) 41°·58.

We extract the observations of the solar and terrestrial radiation entire, principally because so few observations have been made pub-

lic, or even we believe instituted, with the Actinometer, an instrument which we are glad to see appreciated by distant observers.

SOLAR RADIATION.					REMARKS.
DAY & HOUR.	2	3	4	WEATHER.	These observations were made during the warmest and brightest days of July, on the Montreal mountain, place of observation 307 feet above St. Lawrence. The <i>first</i> column gives the day and hour; the <i>second</i> , the indication of Sir John Herschel's Actinometer,—by Robinson, London,—mean of three observations; the <i>third</i> , the temperature of air in the shade, Thermometer 5 feet above the earth; the <i>fourth</i> , the indication of the Thermometer placed in the sun on garden mould, not blackened, after an exposure of ten minutes. N.B. This Actinometer was, in Sept. 1837, compared on the same spot, with one in the possession of Dr. Daubeny, of Oxford, England, and gave similar indications.
July 19	A.M.				
	10,30	19.50	68	114	
	11,00	17.62	69	122	
	11,30	19.50	69.5	118	
	Noon.	20.00	71	120	
	12,30	20.00	73	116	
	1,30	20.00	73	
26	Noon.	14.67	71	94	
27	4 P. M.	17.07	77	
28	Noon.	19.05	77	98	Clear.
3	P. M.	19.50	78	101	
29	Noon.	19.80	83	105	
Aug. 2	Noon.	17.30	73	100	
TERRESTRIAL RADIATION.					The <i>first</i> column indicates the 24 hours during which the observation was made, reckoning from 9 A.M. of one day to 9 A.M. of the next; the <i>second</i> gives the <i>max.</i> of the Thermometer in the shade, for the same period; the <i>third</i> , the <i>min.</i> ; the <i>fourth</i> , the indications of a register spirit Thermometer, placed on a grass plat and exposed freely to the heavens during the night. All these instruments are made by first artists, British, and carefully compared with standards.
DAY.	2	3	4	Wind	
July..	18 to 19	69	60	50	NW. Clear.
	19 to 20	73	54	50	do. do.
	22 to 23	71	55	50	N. do.
	26 to 27	73	61	49	SE. do.
	27 to 28	78	69	62	SW. do.
	28 to 29	80	70	65	SW. Showery.
Aug..	6 to 7	76	69	63	SW. Clear.

METEOROLOGICAL OBSERVATIONS FOR MAY, 1840.

Chiswick.—May 1. Slight haze: fine. 2—4. Hot and dry with easterly wind. 5. Overcast. 6, 7. Slight haze. 8. Heavy showers: fine. 9. Rain with sultry intervals. 10. Cloudy: sultry. 11. Drizzly. 12. Overcast. 13. Rain: sultry. 14. Cloudy and fine. 15. Heavy rain with thunder. 16. Cloudy: showery: hail shower at 12 $\frac{1}{2}$ P.M. 17. Rain. 18. Cloudy and fine: rain. 19, 20. Cloudy and cold. 21. Clear and cold. 22. Overcast. 23. Clear and fine. 24. Overcast: rain. 25. Cloudy. 26. Rain. 27—30. Very fine. 31. Hot and dry: cloudless.

Boston.—May 1. Cloudy. 2—4. Fine. 5, 6. Cloudy. 7, 8. Cloudy: rain early A.M.: rain P.M. 9. Rain: rain early A.M. 10. Cloudy: rain early A.M. 11. Rain: rain early A.M. 12. Cloudy: rain early A.M. 13. Rain. 14. Cloudy: rain with thunder and lightning P.M. 15. Rain. 16, 17. Cloudy: rain A.M. and P.M. 18, 19. Cloudy. 20. Cloudy: rain P.M. 21, 22. Stormy. 23, 24. Cloudy. 25. Stormy: rain A.M. 26. Rain: rain A.M. 27. Cloudy. 28. Fine. 29. Stormy. 30, 31. Fine.

Applegarth Manse, Dumfries-shire.—May 1, 2. Beautiful day. 3. The same: Thermometer in shade 75°. 4. Very dry and warm till P.M. 5. Very droughty. 6. The same increased: cloudy. 7. Slight showers all day. 8. The same A.M.: cleared up. 9. Slight showers early A.M. 10. The same: thunder P.M. 11. Rain heavier. 12. Rain nearly all day. 13. Fair. 14. Showery P.M.: thunder. 15. Occasional showers: thunder. 16. Rain preceding night: clear day. 17. Rain in the night: fair. 18. Fresh and cool. 19. Dry and rather boisterous. 20. Dry and more moderate. 21. Very droughty: clear sky. 22. Calm and warm. 23. Showery: high wind. 24. Showery. 25. Showery, and very high wind. 26. Fresh and showery. 27. Fine A.M.: wet P.M. 28. Very wet till P.M. 29, 30. Fine growing day. 31. Variable: bright at midday: wet evening.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTSON; by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. VALL at Boston, and by Mr. DUNBAR at Applegarth Manse, Dumfries-shire.

Days of Month. 1840. May.	Barometer.				Thermometer.				Wind.				Rain.			Dew point. Lond.: Roy. Soc. 9 a.m.		
	Lond.: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire. 9 a.m.	8½ p.m.	Lond.: Roy. Soc. Self-register.		Chiswick. 9 a.m.	Dumfries- shire. Max. Min.	Chiswick 9 a.m.	Bost. shire.	Dum- fries- shire.	Lond.: Roy. Soc. 9 a.m.	Chiswick.		Boston.	Dumfries- shire.
		Max.●	Min.				Max.	Min.										
1.	30.356	30.343	30.325	29.80	30.25	30.23	48.7	48.2	67	43	49.0	71	45½	49.0	43	45½	50	
2.	30.288	30.290	30.194	29.82	30.21	30.14	55.4	45.2	74	42	53	72	43	53	42	43	52	
3.	30.172	30.161	30.109	29.72	30.14	30.10	59.3	46.3	74	43	55	75	44	55	43	44	49	
4.	30.126	30.105	29.994	29.68	30.05	30.00	58.3	48.7	70	41	58.5	73	49½	58.5	41	49½	49	
5.	29.912	29.924	29.868	29.40	29.96	29.90	55.7	45.4	66	46	52	62	45	52	45	45	49	
6.	29.888	29.903	29.800	29.36	29.87	29.83	54.7	49.0	68	49	50	52	40½	50	40½	40½	51	
7.	29.760	29.761	29.702	29.22	29.70	29.64	57.3	49.7	68	50	51	46½	42½	51	46½	42½	51	
8.	29.706	29.698	29.646	29.07	29.57	29.53	59.7	54.7	70	51	55	50½	41½	55	41½	41½	54	
9.	29.524	29.525	29.425	29.07	29.50	29.53	55.0	54.0	71	51	55	50	42½	55	42½	42½	53	
10.	29.402	29.474	29.392	28.86	29.55	29.57	59.8	53.3	72	50	52	51	38	52	38	38	58	
11.	29.534	29.644	29.533	28.95	29.60	29.67	52.8	51.6	55	50	49	54	39	51	39	39	53	
12.	29.670	29.694	29.657	29.14	29.65	29.68	53.7	51.2	62	48	51.5	50	40	51.5	40	40	52	
13.	29.662	29.702	29.654	29.21	29.65	29.58	53.3	49.8	67	49	53	56	43	53	43	43	52	
14.	29.678	29.682	29.562	29.09	29.50	29.40	59.3	51.9	66	50	55	63½	44	55	44	44	55	
15.	29.414	29.442	29.404	28.79	29.29	29.23	53.7	53.2	60	49	53.5	64	48½	53.5	48½	48½	52	
16.	29.404	29.413	29.379	28.73	29.14	29.16	57.8	51.4	65	47	56	58½	48	56	48	48	53	
17.	29.374	29.469	29.387	28.76	29.24	29.38	55.7	50.2	62	46	55	53½	45½	55	45½	45½	52	
18.	29.626	29.689	29.883	29.06	29.55	29.89	55.8	48.3	62	45	43	53½	42	45	42	42	51	
19.	30.080	30.132	30.060	29.60	29.93	30.01	47.3	46.6	56	34	45	52	42	45	42	42	49	
20.	30.052	30.138	30.060	29.52	29.96	30.05	52.8	41.3	60	40	53	58½	46	53	46	46	48	
21.	30.216	30.305	30.202	29.67	30.18	30.21	47.8	41.6	54	42	46	63	35½	46	35½	35½	40	
22.	30.344	30.393	30.361	29.83	30.25	30.20	48.8	44.3	64	36	49	60½	36½	49	60½	36½	45	
23.	30.356	30.361	30.229	29.75	30.10	29.89	57.7	44.2	71	51	58.5	56½	52	58.5	52	52	46	
24.	30.172	30.182	29.953	29.47	29.82	29.57	61.5	53.3	67	55	61	60	45	61	45	45	53	
25.	29.808	29.959	29.684	29.29	29.40	29.65	58.8	56.3	66	45	62	54	46	62	46	46	55	
26.	29.844	29.962	29.764	29.26	29.63	29.73	54.6	49.4	65	38	48	56	41	48	41	41	50	
27.	30.050	30.041	29.960	29.43	29.77	29.70	56.7	45.6	74	39	57	59	41	57	41	41	51	
28.	29.926	29.960	29.887	29.25	29.58	29.65	60.8	50.9	77	39	61.5	59	41	61	41	41	52	
29.	30.088	30.284	30.120	29.37	29.84	30.07	60.7	55.0	70	44	60	59½	44	60	44	44	54	
30.	30.398	30.388	30.365	29.76	30.10	30.05	61.4	51.4	73	48	62	57	40	62	40	40	56	
31.	30.400	30.379	30.326	29.71	30.00	30.00	63.8	56.4	80	44	67	64	50	67	50	50	56	
Mean.	29.911	29.948	29.867	29.33			56.3	49.6	67.00	45.32	54.2			Sum. 2.329	2.20	2.46	Mean. 51.1	

THE
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[THIRD SERIES.]

AUGUST 1840.

XIII. *On the Theory of the dark Bands formed in the Spectrum from partial Interception by transparent Plates. By the Rev. BADEN POWELL, M.A., F.R.S., F.G.S., F.R.Ast.S., Savilian Professor of Geometry in the University of Oxford*.*

(1.) **T**HE phænomenon of peculiar dark bands crossing the prismatic spectrum, when half the pupil of the eye (looking through the prism) is covered by a thin plate of any transparent substance, the edge being turned *from the violet towards the red* end of the spectrum, was first described by Mr. Fox Talbot in 1837 (Lond. and Edinb. Phil. Mag. and Journal of Science, vol. x. p. 364.), who showed that these bands are due to the interference of the two halves of each primary pencil, one of which is retarded by the plate.

(2.) Sir David Brewster has given various new modifications of these experiments (British Association Reports, vol. vii. Trans. of Sections, p. 13.), the most material of which tend to show that the effect is fully produced *only* when the plate is in the position just described, and diminishes and disappears as it revolves in its own plane; the same observation being also extended to the case of the spectra formed by interference from grooved surfaces, or gratings.

(3.) The explanation given by Mr. F. Talbot accounts for *the production of the bands simply*, but assigns no reason why the interception must take place *on one side* more than the other. That it does so, is considered by Sir David Brewster as indicating an entirely *new property of light*; having reference to the different sides of the pencil related to their position of greater or less refrangibility, and which he has not inexpressively termed a peculiar "polarity."

* Communicated by the Author.

Phil. Mag. S. 3. Vol. 17. No. 108. Aug. 1840.

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(4.) My attention was drawn to the subject in the course of last summer, when I repeated the experiments, and devised several new modifications with reference to an explanation which it appeared to me was supplied by the undulatory theory; to these investigations I referred briefly at the Birmingham meeting of the British Association, 1839. For several reasons (on which I need not here enter) I have delayed publishing any details; nor should I do so now, but that having learned that Mr. Airy has recently pursued the research to many entirely new conclusions*, I am anxious to put on record the few points I have been able to establish, and to vindicate my views from misconceptions to which they have been exposed.

(5.) The following distinctions are important to be borne in mind with reference to the explanation of the phænomena.

In these experiments we have to consider the different elementary pencils of which the spectrum, as presented to the eye, is formed; and with respect to each of these, in the case of the *prismatic* spectrum it is easily seen that the edge of the plate intercepts *that half which lies towards the edge of the prism.*

In the *interference*-spectrum (according to Fraunhofer's method), the spectra are formed one on each side of the axis, with their violet ends towards it. The edge of the plate in this case must always intercept *that half of each primary pencil, which after passing the focus lies NEAREST to the axis.*

(6.) I have found that with the same prism the intercepting plate must be within limits of thickness, which differ according to the substance of the plate, and with the same plate the character of the bands differs with the medium of which the prism is formed. These differences appear to depend on the refractive and dispersive powers of the substances.

(7.) With a prism of flint glass and a plate of mica, the greatest thickness which can be used may be about the $\frac{1}{100}$ th of an inch. In this case the bands appear fine and numerous, and it seems only in consequence of their increase in number that they cease to be distinguishable when the thickness is increased beyond this.

If we use less thicknesses (such as those into which mica is easily split) the bands become broader and fewer, and at length faint and ill-defined. It is perhaps not possible to distinguish them if fewer than four or five are formed throughout the spectrum. The bands are never very dark; showing that only a portion of the rays is concerned in their formation.

[* A notice of Mr. Airy's paper on this subject will be found in our report of the proceedings of the Royal Society for June 18, 1840.—EDIT.]

(8.) When the plate is very thin, another set of appearances presents itself.

On splitting a piece of mica to such a tenuity that only a few indistinct broad bands were barely visible with a flint-glass prism, I observed at the same time *another set of very fine but extremely faint bands*, evidently independent of the former.

(9.) When the film of mica was still thinner, the broad bands ceased to appear altogether, and *only the fine set* were visible. To show *these* bands the films must be so thin as to be nearly iridescent: it is difficult to succeed in tearing them off sufficiently fine. I have sometimes used drops of water between glass plates pressed hard together. These bands are always very faint; but they are somewhat more conspicuous with prisms of the more dispersive oils, and always require a strong light to be seen.

(10.) There is, however, a more remarkable circumstance connected with *this set of bands*; *they continue to be formed when the edge of the thin film is towards the thicker side of the prism.*

(11.) In pursuing the *theoretical* explanation, we have to consider the conditions which may affect the rays situated towards the opposite sides of those primary homogeneous pencils, into which the incident beam of light is separated, and which converge in the eye to form the several points in the spectrum, both in the case of the prism, and of interference from grooves or gratings.

Now in either case a distinction of this kind is deducible from the wave-theory, on comparing the length of undulatory route of the two extreme rays of any primary pencil; from which it appears that *one of these rays is always more retarded than the other*, as well in the prismatic as in the interference spectrum: that side of the pencil which is *previously the least retarded, being that to which the plate is applied* in the original form of the experiment. This distinction, combined with the general principles of explanation at first referred to, appear to me not only sufficiently to account for the *ordinary* phenomena, but in *my modification* of the experiment to assign a reason why a similar effect should be produced on the *opposite* side.

(12.) With regard to the mathematical investigation, in the case of the prism, without going into a formal discussion, it is sufficient to observe, that on the principles of mathematical optics, when a diverging pencil of homogeneous light is refracted through a prism in the position of minimum deviation,

the emergent pencil will originate from a geometrical focus, which is *not a single point*, but a *caustic*, whose *convexity* is towards the edge of the prism. Hence, on the principles of the wave-theory, it follows that the side of the pencil which lies *towards the edge* of the prism is that which undergoes less *retardation*, or has the *shorter* undulatory route; this difference varies slightly for the different primary rays.

I had arrived at this conclusion by a different approximate method, when in some correspondence the Astronomer Royal pointed out to me the above view of the problem, as connected with his investigations "on the light in the neighbourhood of a caustic." (Camb. Trans., vol. vi. part 3*.)

(13.) With respect to the interference-spectrum, we have only to follow out the investigation given in Mr. Airy's tract (Arts. 80, 83.) (as that gentleman has suggested to me) in the following manner.

Taking the focus as the origin and the axis of the object-glass as the axis of x , let $x y$ be the coordinates of any point in the wave, $a b$ those of a point in the focal image on the same side of the axis, then the radius of the wave being c , we have

$$c^2 = x^2 + y^2;$$

and expanding and neglecting powers of y above the third, we find

$$x = c - \frac{y^2}{2c}.$$

The distance g from $x y$ to $a b$ will be

$$g = \sqrt{(x-a)^2 + (y-b)^2};$$

here performing the various expansions, and for brevity writing $e^2 = (c-a)^2 + b^2$, we at length obtain (going to the third power of y)

$$\rho = e \times \left\{ \begin{aligned} &1 + \frac{1}{2} \left\{ \frac{2b}{e^2} y + \frac{a}{c e^2} y^2 \right\} \\ &- \frac{1}{8} \left\{ \frac{-2b}{e^2} y + \frac{a}{c e^2} y^2 \right\}^2 \\ &+ \frac{1}{16} \left\{ \frac{-2b}{e^2} y \right\}^3 \end{aligned} \right\}.$$

The terms involving the second power of y have the same value on each side of the axis, and those depending on the third power are found to be

[* See Lond. and Edin. Phil. Mag. vol. xii. p. 452.—EDIT.]

$$\left. \begin{aligned} & -\frac{1}{8} \left\{ \frac{-4ba}{c^4} y^3 \right\} \\ & +\frac{1}{16} \left\{ \frac{-8b^3}{c^6} y^3 \right\} \end{aligned} \right\} = \frac{by^3}{2c^4} \left\{ \frac{a}{c} - \frac{b^2}{c^3} \right\}.$$

Here, if $\frac{a}{c} < \frac{b^2}{c^3}$, (which is the case, the image being formed in the focus, so that in fact we might assume $a = 0$,) it follows that when b and y have the same sign, this expression will be negative; that is, for the ray which has b and y on the same side, or is *nearest the axis after passing the focus*, the route will be the *shortest*. The difference will be very small, and will vary slightly for the different rays of the spectrum.

(14.) This difference of retardation in the several rays of each primary pencil, combined with the obvious principle laid down by Mr. F. Talbot, appears to me to supply an explanation of the phenomena.

The whole effect in these experiments is made up of two parts, the original retardation, and that superinduced by the plate. If the previously *least* retarded ray be intercepted, we take the *difference*, if the *most* retarded, the *sum* of the two effects.

When we apply the plate, the whole resulting retardation may fall within the limits, (before mentioned, § 6.) or not, according to the magnitude of the two retardations, and according as we take their sum or difference. If it be beyond the limits for one portion of the pencil, it may be within them for another.

In general, in the original form of the experiment, that is, for plates of ordinary thickness, the *difference* falls *within* the limits, though the *sum* is *beyond* them, for all portions of the pencil. But with a very thin plate, the *sum* may also be *within* the limits for *those parts* of the pencil whose difference of retardation is small: Or, in other words, with plates of a *certain thickness*, the retardation is too great to give bands with any portion of the pencils, when the plate is applied to the previously *most* retarded side: but it will give bands with some portion when applied to the previously *least* retarded side.

On the other hand, if an *extremely thin* plate be applied to the *most* retarded side, it will still give bands with one portion of the pencils, as well as when applied to the least retarded side with other portions.

Oxford, July 5, 1840.

XIV. *On the Potatoe Spirit Oil of the French Chemists.* By JAMES APJOHN, M.D., M.R.I.A., Professor of Chemistry in the Royal College of Surgeons, Dublin*.

IN December 1838, I received from my friend Mr. Scanlan a specimen of an oily fluid which had been given him by Mr. Bowerbank, an eminent London rectifier, and which the latter gentleman had found in small quantity in the faints or weak spirit drawn off towards the close of the rectification of common whisky. Shortly previous to this time, Mr. Coffey, the inventor of the celebrated patent still, had observed the same substance at the extensive distillery of Sir Felix Booth; and upon coming over to Dublin, and visiting the establishment of Mr. Busby at Blockpitts in this city, Mr. Scanlan had the satisfaction of recognizing this same oil in the faint vessel, constituting a thin stratum resting upon the surface of the remainder of the fluid.

The oil obtained from Mr. Busby's concern had a reddish-brown colour, owing to dissolved vegetable matter, and its specific gravity was $\cdot 8401$, that of the faints on which it rested being $\cdot 9269$. Shaken in a graduated tube with an equal bulk of water, its volume was reduced 20 per cent., and the water upon distillation yielded alcohol. To insulate the oil, therefore, the following method was adopted.

The fluid obtained from the faint receiver was first washed with an equal bulk of water; then shaken in a bottle with an equal weight of pulverized and anhydrous carbonate of potash, and finally distilled from a glass retort, the condensation being effected by Liebig's tube refrigeratory. It began to boil at 262° , after which the temperature rose gradually until it became 268° , at which it continued until the whole of the oil was nearly over. The fluid first drawn off was set apart, as still containing alcohol, and that alone reserved for further purification which distilled over at 268° . This portion was redistilled. The ebullition commenced a little over 267° , and in less than a minute rose to 268° , at which point it continued until the rectification was nearly completed. The first and last portions being rejected, the middle portion, or that which came over at 268° , was set apart for experiment.

The oil thus procured is a perfectly colourless liquid, destitute of all viscosity. The specific gravity is $\cdot 8138$, and, as has been already observed, it boils steadily at 268° ; cooled to -6° it does not congeal. With rectified spirit it is miscible in all proportions, its specific gravity being thus aug-

* Communicated by the Author.

mented, and its boiling point lowered. It is immiscible with water, but nevertheless when agitated with this liquid, it absorbs an appreciable quantity of it. It has a pungent and peculiar odour, and a sharp biting taste, somewhat similar to that of the oil of cloves. When gently heated it readily takes fire upon approaching to it a lighted taper, and burns with a clear flame unaccompanied by smoke. It is an excellent solvent for the fats, and also for resinous substances. Camphor, for example, is readily dissolved by it; and the same may be said even of copal, if a gentle heat be applied. Potash is taken up by it in considerable quantity, oil of vitriol gives it a crimson colour. To determine its composition the following experiments were made:

(1.) 4.24 grains of the oil burned in the usual manner with oxide of copper gave of water 5.06 grains, and of carbonic acid 10.42 grains.

(2.) 7.71 grains gave of water 9.22 grains, and of carbonic acid 19.12 grains.

(3.) 6.63 grains gave of water 8.05 grains, and of carbonic acid 16.26 grains.

The following are the results deducible from these experiments:

	(1.)	(2.)	(3.)
Carbon.....	67.96	68.59	67.84
Hydrogen	13.25	13.28	13.48
Oxygen	18.79	18.13	18.68
	<hr/>	<hr/>	<hr/>
	100	100	100

The means of the numbers yielded by the three experiments are given underneath in column (a.). The numbers in column (b.) are the quotients of the corresponding ones in (a.) divided by the respective atomic weights of carbon, hydrogen, and oxygen, and those in column (c.) are others in the same ratio with the quotients.

	(a.)	(b.)	(c.)
Carbon	68.13	11.132	4.804
Hydrogen ...	13.33	13.330	5.753
Oxygen	18.54	2.317	1.00
	<hr/>		
	100.		

The inspection of the latter shows that the most probable formula for the oil is $C_5 H_6 O_1$, which would give the following parts per cent.

Carbon	68.60
Hydrogen.....	13.45
Oxygen.....	17.95
	<hr/>
	100.

Assuming this formula as the true one, the deficiency in the carbon experimentally determined is not greater than what usually takes place; but the error in the hydrogen, though trifling in amount, being upon the opposite side to that on which it usually occurs, it became expedient to resort to some method of verification. The specific gravity of the vapour of the oil was therefore taken according to the well-known method of Dumas.

The weight of glass ball filled with dry air, the pressure being $30\cdot324$ and temperature $48^{\circ}\cdot5$, was $914\cdot86$ grs.

The ball was sealed at 364° , and then weighed $917\cdot78$ grs. Hence $917\cdot78 - 914\cdot86 = 2\cdot92$ grains is the excess of the weight of vapour in ball over that of the air displaced.

The capillary end of the beak attached to the ball having been broken under mercury, it was ascertained by the amount of this metal which flowed into the ball, that its capacity was $16\cdot76$ cubic inches, which, at a pressure = 30, and a temperature = 60° , will (supposing it air) become $16\cdot76 \times \frac{448+60}{448+48\cdot5} \times \frac{30\cdot324}{30} = 17\cdot333$ cubic inches. From this must be subtracted $\cdot1$ cubic inch which was found to remain in the balloon, so that the bulk of air excluded by the vapour, when reduced to the mean temperature and pressure, was $17\cdot233$ cubic inches, whose weight = $5\cdot344$ grains. Hence $2\cdot92 + 5\cdot344 = 10\cdot264$ is in grains the weight of the vapour.

The capacity of the glass balloon at $48^{\circ}\cdot5$ being $16\cdot76$ cubic inches, it will, owing to the expansion of glass, become at 364° $16\cdot843$ inches. This therefore is the volume of the vapour and bubble of air at 364° . But the volume of the latter being $0\cdot1$, it will at 364° become $0\cdot16$. Hence the true volume of the vapour at $364^{\circ} = 16\cdot843 - 0\cdot16 = 16\cdot683$; so that

$16\cdot683 \times \frac{448+60}{448+364} \times \frac{30\cdot324}{30} = 10\cdot549$ is the volume of the vapour reduced to a temperature = 60° and pressure = 30. But as this weighs $10\cdot264$ grains, 100 cubic inches of it would weigh $97\cdot298$ grains. The specific gravity therefore of the vapour is $\frac{97\cdot298}{31\cdot0117} = 3\cdot137$.

Now if the composition of the oil be $C_5 H_6 O$, the specific gravity got by adding together the products of the densities of the vapours of the different elements by the number of atoms of each would be $3\cdot072$. But we have here so close a correspondence between experiment and calculation, that no doubt can remain as to the correctness of the basis on which

the latter rests, or that the true formula for the oil is that already assigned.

These experiments were made in the winter of 1839, and at the time I had concluded them I was under the impression that the oil, to which they relate, was a new substance, or rather one which had not been previously described. In some time after, however, upon looking over the second part of Mr. Graham's Elements of Chemistry, which had been sent me by the author, I was much surprised at finding at page 134, in a table of the volumes of atoms in the gaseous state, mention made of a substance under the designation of "oil of the ardent spirit from potatoes," to which he attributed the very same formula and density of vapour which I had found to belong to the oil found in grain-whisky, in the examination of which I had been recently engaged.

Anxious to investigate the matter further, and to ascertain if the two oils were certainly the same, I looked into Berzelius's System, and the 5th volume of the *Traité de Chimie, appliquée aux Arts*, by Dumas, devoted to the subject of organic chemistry, but could not find any mention in either of the essential oil from potatoe spirit alluded to by Graham. Upon, however, turning to Dr. Thomson's recent volume on vegetable chemistry, I found, page 481, a notice of this fluid, and references to the 30th and 56th volumes of the *Annales de Chim. et de Phys.* in the former of which its origin and properties are described by Pelletier, and in the latter of which its analysis is detailed by M. Dumas. The properties I find ascribed by these chemists to the potatoe spirit oil are precisely those which belong to that which I have examined from corn-whisky, the only difference being that Pelletier represents its specific gravity as $\cdot 821$, whereas I have found that of the oil I obtained from Mr. Scanlan but $\cdot 813$, a difference, however, easily explained by the circumstance of his not having taken the necessary steps for rendering the fluid he examined perfectly free from water and alcohol. The analytic results also of M. Dumas are nearly identical with mine, approaching however more nearly, as might indeed be expected from his great skill in this department of chemistry, to the formula $C_5 H_6 O_1$ which he adopts, and which Professor Graham has taken from his memoir. I may lastly mention, as a very unusual coincidence, that the specific gravity of its vapour, as obtained by Dumas, is $3\cdot 147$, or but unity in the second place of decimals greater than what has resulted for the corn-spirit oil from my experiments.

We thus arrive at the conclusion, that the two fluids are identical, or that the oil which has hitherto been considered

as peculiar to potatoe spirit, occurs also in that which is developed during the fermentation of grain. From this latter source it admits of being procured in great quantity. When first observed by Mr. Coffey at Sir Felix Booth's, there was an inch of it in the faint receiver, and from the diameter of the vessel he estimated its total amount at 50 gallons. This is the quantity produced at that establishment every fortnight, the excise laws compelling the distiller to distil and brew alternately, and a week being generally consumed in each process.

The whisky manufactured some years ago contained a considerable quantity of this oil, and owed to its presence a great deal of the pungency of taste by which it was distinguished.

From its high boiling point, and the nature of the stills at present used, but a very small portion of this substance now passes over, and hence the reason why the spirit at present made is, as compared with the product of the old processes, less disagreeable to the palate, and probably less injurious to the constitution. It is no doubt owing to the same cause, viz. an improvement in the process of distillation, that this oil has at length been noticed in the distillers' faints. Upon the old system of manufacture the greater portion of it was driven over, and was held dissolved by the spirit into which it was thus introduced; but with the modern stills, particularly that devised by Mr. Coffey, nothing having so high a boiling point as this oil can by possibility pass into the part of the apparatus where the spirit is condensed. With respect to the manner in which the substance originates, whether it exists ready-formed in the materials subjected to fermentation, or is a product of the process, I am not aware of any facts calculated to decide such a question. As, however, it is found in the fermented wash of both corn and potatoes, it may be presumed to be derived from the starchy principle, which is common to both.

The potatoe spirit oil, as it has hitherto been called, has I find of late attracted much attention. Pelletier from some rough experiments upon it with acids, threw out the conjecture that it was more analogous to alcohol than to the true volatile oils, and this opinion would seem to have been in some degree adopted by Dumas. More recently (*Ann. de Chimie et de Phys.*, Jan., 1839) M. Auguste Cahours has revived this opinion, and concluded it to be one of the group including alcohol, pyroxylic spirit, and acetone, and has even succeeded in procuring from it a carbo-hydrogen, in which the elements are, as usual, associated in the ratio of atom and atom. M.

Cahours represents potatoe oil by the formula $C_{10} H_{11} O_1 + H O$, which makes it as to composition perfectly analogous to ordinary alcohol. The carbo-hydrogen $C_{10} H_{10}$ he insulated by distilling the oil from anhydrous phosphoric acid. He calls it amylene, and finds the specific gravity of its vapour to be 4.904, so that an atom of it gives but one volume of vapour, a circumstance in which, as Cahours observes, it agrees with Dr. Kane's mesitylene, $C_6 H_3$, but differs from the carbohydrogens which occur in alcohol and pyroxylic spirit. By acting on potatoe oil with sulphuric acid and chlorine, Cahours obtained compounds corresponding perfectly with those yielded by alcohol when similarly treated. These researches give additional interest to the discovery of this fluid in grain-fermented wash, and in such quantity as to be much more than adequate to meet any demand for it with a view to the interests of science.

I may here observe, that I should have long since presented this notice to the Academy, but for the following reasons.

There is another oily substance having, at common temperatures, the consistence of butter, which is long known to exist in the farts of grain spirit, and in smaller quantity in the spirit itself. Upon looking through systematic treatises on chemistry, I found that this oil had been but very imperfectly described, and that, in particular, no experiments had been made with the view of determining its composition. I had therefore resolved to submit it to an accurate examination and analysis, and to keep back what I had ascertained in reference to the fluid oil until I had completed my investigations into the nature and constitution of that which is a soft solid at common temperatures. In this investigation I had made some progress, when my attention was directed to a paper by Liebig and Pelouze, in the 63rd volume of the *Ann. de Chim. et de Phys.*, in which, with their usual ability, they develop the nature of a butyraceous or fatty product which they had received from M. Deleschamps, and which comes over towards the close of the process of distilling wines with a view to the production of *eau de vie* or brandy. This oil they found to be a mixture of an acid which they called œnanthic acid, and of a compound of this acid with the oxide of æthyle, that is of œnanthic acid and œnanthic æther. Upon perusing this paper I saw at once, from the experiments I had already made, that the fatty oil of grain spirit was identical with this mixture, with the exception that some third oleaginous material was present, which Liebig and Pelouze had not found in what they had operated upon. Upon this third substance I have made some experiments, the results of which I shall probably at some

future period submit to the Academy. I have resolved, however, no longer to defer giving publicity to my experiments identifying the fluid oils of grain and potatoe spirit, having had my attention drawn by Dr. Kane to a recent volume of Poggen-dorff's *Annalen*, containing a paper by M. Mulder, in which I find myself anticipated on the other point; and the butter of corn spirit is satisfactorily shown to be what I had concluded it to be, not entirely from my own experiments, but from a comparison of them with the researches of Pelouze and Liebig. Mulder also notices the third principle which is associated with the œnanthic acid and œnanthic æther, and describes it under the name of *oleum siticum*. The object therefore of the present communication is much more limited than it was originally intended to be, professing only to announce the detection of the potatoe-spirit oil of Pelletier and Dumas in fermented infusions of the mixed grains employed by the distiller. But as Mulder conceived his discovery of sufficient interest to justify him in giving it to the scientific world, I shall, I trust, be pardoned for bringing an analogous fact under the notice of the Academy.

XV. *Observations on the Climate of Italy and other Countries in ancient times*.*

A VAGUE notion seems to have prevailed for some time past among persons conversant with ancient authors, that the climate of Europe in the classical ages of Greece and Rome must have been considerably colder than at the present day. Latterly this question has been taken up by two philosophers, who from a consideration of the vegetation have come to a different conclusion. Most persons probably have read the interesting essay by Arago in the *Annuaire* for 1834, who states that several of his facts have been borrowed from the writings of Schouw. The conclusions drawn by these writers are probably in the main correct; but some of the facts stated by them appear to require modifications, which are more fully explained in the following pages.

I. *Vegetation of Ancient Italy: the beech, the date-palm, the olive.*

It has been said that Virgil speaks of the beech as growing in the neighbourhood of Rome; whereas now the climate is too hot for that tree, which is not found till we reach a considerable height on the Apennines. In fact, Tenore (*Cenno*

* Communicated by the Author.

sullo *Geografia Fisica e Botanica del Regno di Napoli*, p. 60*.) places the region of the beech in Southern Italy, at from 400 to 600 toises above the level of the sea. Virgil, however, in reality never speaks of the "fagus," generally supposed to be the beech, as growing near Rome. That tree is mentioned twice in the *Georgics* (i. 173. and ii. 71.), but in a way from which nothing can be concluded with regard to its locality; and once in a similar way in the *Eclogues* (iii. 12.): there are, however, two other passages where the indication of the locality is more precise. The first is the well-known passage of the first *Eclogue*. It is too obvious to require any discussion, that in this instance the scene is laid in the territory of Mantua. The second passage (*Ecl.* ix. 9.) refers to the same country, and specifies the position of the beeches still more clearly as in the plain, but not far from the foot of the hills,

"————— quà se subducere colles
Incipiunt, mollique jugum demittere clivo,
Usque ad aquam et veteres, jam fracta cacumina, fagos."

With the exception of a passage in the 2nd *Eclogue* (l. 3.), where the scene is laid "Siculis in montibus," these are, I believe, the only occasions on which the "fagus" is mentioned by Virgil; nor in any of them is it connected with the neighbourhood of Rome. Still, however, it is remarkable that Virgil should speak of the "fagus" as growing in the plains of Northern Italy, while in the present day we must ascend the adjacent mountains to a considerable height before we find it. The lower limit of the beech on the southern side of the Alps (in the Valtelline and the Veronese) is placed by Schouw (*Pflanzen-Geographie*, p. 199.) at 2000 Parisian feet above the level of the sea, while the altitude of the plain of Lombardy is only 400. It would appear therefore that in Northern Italy the lower limit of the beech has ascended 1600 feet since the time of Virgil.

The position of Virgil's "fagi" in the plain near the foot of the hills, is in accordance with a passage of Pliny, where he puts the fagus among the mountain trees, that also descend into the plain. (*Hist. Nat.* xvi. 30.)

In another passage (xvi. 15.) Pliny states that fagi formerly existed on the site of the temple of Jupiter Fagutalis within the precincts of the city of Rome. Brocchi, in his *Physical Map of Rome*, at the earliest period of its existence, lays down the *Lucus Fagutalis* on the Esquiline, not far from the *Agger of Servius Tullius*. The mean height of the Esquiline above the Mediterranean does not exceed 200 Parisian feet, and the highest point of the *Agger* only reaches 237, accord-

[* See L. & E. Phil. Mag. vol. iv. p. 276.—EDIT.]

ing to the measures of Schouw (Brocchi *Suolo di Roma*, p. 211.): consequently this grove of beeches must have existed at a height of little more than 200 feet above the sea.

I am aware that it is denied by some that the *Fagus* of Virgil is our beech: but even those persons admit that the *Fagus* of Pliny is the beech; and indeed in the description of that author (*Hist. Nat.* xvi. 7.), the triangular coat of the gland, the smoothness of the leaf, and the care with which the *fagus*, though bearing an edible and sweet gland, is distinguished from the trees bearing real "glandes," that is acorns, seems pretty decisive on this point.

The only argument urged (*Gardener's Magazine* for January, 1839, pages 10 and 19.) against the *Fagus* of *Virgil** being the beech (beyond the fact now under discussion of its not being found in the plains of modern Italy), is founded on an erroneous reading of a passage in the *Georgics*, according to which Virgil is made to speak of engrafting the *fagus* on the *castanea* or chestnut. But in the text of the best modern editions, Virgil says no such thing. The passage (*Georgic.* ii. 69.) stands in Heyne thus:

"Inseritur vero et fœtu nuci arbutus horrida;
Et steriles platani malos gessere valentes;
Castaneæ fagus, ornusque incanuit albo
Flores pyri....."

It is evident that the poet speaks of engrafting the chestnut on the beech, and not the beech on the chestnut. Whether such a graft be really possible, I do not know; but it is not more out of the way than the other operations of a similar kind alluded to in this passage.

The statements of Pliny regarding the existence of the beech in the neighbourhood of Rome, are remarkably confirmed by Theophrastus, who speaking of the plain of Latium,

* I say Virgil, for the most serious difficulty about the meaning of the word "*Fagus*" arises from a passage of Cæsar (*Bell. Gall.* v. 12.), who speaks of this tree as not found in Britain. I may observe, that it is not more extraordinary that Cæsar should assure us that the beech was not found in Britain, than that Herodotus (ii. 77.) should say that the vine was not grown in Egypt, where, however, existing monuments, (Rosellini, *Mon. Civil.*, t. ii. p. 366.) and various passages of the Pentateuch (Genesis, xl. 9. Numbers, xx. 5.), show it to have been cultivated from the earliest period. But I have only to do at this moment with the "*Fagus*" of Virgil and Pliny. As to the latter, it is true that the passage, "*dulcissima omnium fagi*," (*Hist. Nat.* xvi. 8.) appears to be a translation of *γλυκύτατα δὲ τὰ τῆς φηγῶν* (*Hist. Plant.* iii. 8.); but on the other hand, while Theophrastus (*Hist. Plant.* iv. 12.) speaks of the *φηγὸς* on the tomb of Ilus, Pliny (*Hist. Nat.* xvi. 8.) uses the term "*Quercus*," which shows that this latter careless and inconsistent writer did not always confound the Greek *φηγὸς* with the Latin "*Fagus*."

says (*Hist. Plant.* iii. 10.), ἡ μὲν πεδινὴ (τῶν Λατίνων) δάφνην ἔχει καὶ μυρρίνην καὶ ὀξύνην θαυμαστήν. The ὀξύα of Theophrastus appears clearly from his description to have been the beech; and ὀξύα is still the name of that tree in modern Greece, according to Sibthorp. (*Schneider in Theophrast.*, vol. v. *Indic. in voce* ὀξύα.) That the plain of Rome should have produced, together with laurels and myrtles, beeches of wonderful size, is an extraordinary botanical fact, and one of the highest importance for the purposes of the present inquiry. For if we confine ourselves to the circumstances connected with the vegetation of the beech, we should be led to fix the mean temperature of ancient Rome at 3° or 4° centigrade lower than at present*; a temperature by no means suitable to the laurels and myrtles, and equally at variance with other phænomena, as I shall proceed to show.

It is important to be noticed, as bearing on the question of temperature, that Pliny (xvi. 59.) asserts that the chestnut would hardly grow in the immediate vicinity of Rome: "Juxta Romam ipsam castaneæ cerasique ægrè proveniunt,"—doubtless on account of the heat. In the present day the region of the chestnut in southern Italy is placed by Tenore (p. 58.) at from 150 to 400 toises above the level of the sea. In the Abruzzi, the same region is placed by Schouw (*Pflanzen-Geographie*, p. 475.) at from 200 to 600 toises. Now the highest of the seven hills of Rome does not reach 50 toises; and Monte Mario, the most conspicuous summit in the neighbourhood, only attains 74. (Brocchi *Suolo di Roma*, p. 213.) Consequently Rome does not lie within the existing region of the chestnut, and that tree, if planted near it, would probably grow but "ægrè."

That the climate of Italy was not very much colder in ancient times than at present, may, I think, also be inferred from the fact of the date-palm growing there (in the cultivated state), though then, as now, it remained sterile. (Pliny, xiii. 6.) In Italy at present, if we except the garden of a convent at Rome (a very sheltered spot), and also a small tract of coast between Nice and Genoa, which from local causes enjoys a very high temperature, the northern limit of the date is at Terracina. Now the mean temperature of Rome is 15°·5 C., of Naples 17° C. Terracina, lying nearly half-way between them, certainly has more than 16° mean temperature. At Nice the temperature is 15°·5; at Genoa 15°·9. At neither of these places will the

* The lower limit of the beech appears to have ascended from less than 50 to 400 toises above the level of the sea. We may assume, approximately, a decrement of 1° C. in the mean temperature for every 100 toises of elevation.

date-tree grow; but at Bordighiera, which lies between them, in a very warm exposure, it is cultivated rather extensively. We must then put the mean temperature of Bordighiera above 16° , and probably it is above that of Toulon, viz. $16^{\circ}6$. From these circumstances it evidently results that the cultivation of the date-palm in Italy requires a mean annual heat of from 16° to 17° C., and probably nearer the latter. Now the temperature of the southernmost provinces of Italy is not in all likelihood above $17^{\circ}5$, the temperature of Palermo; and consequently had the climate been two centesimal degrees lower in the days of Pliny, the date-tree could not have been cultivated in Italy; certainly not, as he tells us, "vulgò*."

I have argued on the supposition that the words "sunt quidem et in Europâ, vulgòque Italiâ, sed steriles," refer to the date-palm: I think he cannot mean the palmetto, *Chamærops humilis*, as he adds, "Nulla est in Italiâ sponte genita;" and it is difficult to suppose that the *Chamærops* could have been a cultivated plant: yet even in this, as it seems to me, very improbable supposition, the argument would not be affected, since the limit of the *Chamærops* is very nearly that of the date-tree. If we except the tract of coast at Bordighiera, before spoken of, the northern limit of the palmetto in Italy is at Cape Circeii near Terracina†. (Tenore, *Cenno*, p. 68.)

It is observed by Arago, that Virgil speaks somewhere of the rivers freezing in Calabria. He alludes, I presume, to a passage in the fourth Georgic (l. 125, &c.), in which the poet speaks of the success of a gardener in the neighbourhood of Tarentum‡.

"Et cum tristis hyems etiam nunc frigore saxa
Rumperet, et glacie cursus frænaret aquarum,
Ille comam mollis jam tondebat hyacinthi."——

These are evidently merely poetical exaggerations. Horace, writing about the same time, praises Tarentum for the mildness of its winters. (*Od.* ii. 6.)

"Ver ubi longum tepidasque præbet
Jupiter brumas:"

* I am surprised that the learned and accurate Ritter (*Asien.* vol. iv. lib. 3. §. 99.) should think that the date-tree was introduced into Spain by monks from Egypt. "Ferunt (palmæ) et in maritimis Hispaniæ fructum, verùm immitem." Plin. xiii. 6.

† It may be observed in passing, that the *Chamærops* was abundant in Sicily in ancient times as it is now. (Theophrast. *Hist. Plant.* ii. 6. Plin. *Hist. Nat.* xiii. 9.) This fact alone proves that the temperature of Sicily was not 2° C. lower than at present.

‡ The Calabria of the ancients, it will be recollected, was not the province now called by that name; it comprised the peninsula which is cut off by a line from Brundisium to Tarentum.

and what is more precise, boasts of the excellence of its olives. Now this tree is too delicate to endure such winters as those described by Virgil; it will not even bear those of northern Italy, where, however, the freezing of the rivers is a rare occurrence. The conditions laid down by Humboldt for the successful cultivation of the olive are a mean annual temperature from $14^{\circ}5$ to 17° C., a mean temperature of the coldest month not below 5° or 6° C., and of the whole summer from 22° to 23° C.

II. *Palestine, Egypt, the southern limit of the Vine.*

An ingenious argument has been founded upon the simultaneous cultivation of the date-tree and the vine in Palestine at an ancient epoch. The date-tree, it is argued, requires a mean temperature of at least 21° C. to ripen its fruit; and on the other hand, the vine it is said cannot be cultivated for the purpose of making wine at a higher mean temperature than 22° C. It is concluded that the mean temperature of ancient Judæa was between 21° and 22° ; and it is said that this appears to be about its temperature at the present day. One part of these propositions I think open to doubt. Schouw, it is true, agrees with von Buch in placing the southern limit of the vine (at the level of the sea) in the island of Ferro, which has probably from 21° to 22° mean temperature. He states that the vine succeeds only in the northern portions of Barbary; that the cultivation of it in Egypt is insignificant; that at Bushire, on the Persian Gulf, it can only be cultivated in deep ditches to protect it from the sun. Now the mean temperature of Bushire is stated by Humboldt at $25^{\circ}5$; this example, therefore, proves nothing. As to Barbary and Egypt, Mahometan prejudices prevent the making of wine in those countries at the present day; but we know that in ancient Egypt the vine was cultivated on a great scale, and many excellent wines produced. Nor was this by any means confined to Lower Egypt; on the contrary, excellent wine was made in the Thebaid, and particularly at Koptos, in 26° north latitude*, and even in the Great Oasis, still further south†. Theophrastus, as Arago has already observed, speaks of the vine reaching as far as Elephantine‡. But I will be contented with the southernmost limit, where

* Athenæus, lib. i. ἡ δὲ περὶ τοῦ Νεῖλου ἄμπελος, πλείστη μὲν αὐτὴ ὅσος καὶ ὁ ποταμός· καὶ πολλὰ καὶ τῶν οἴνων αἱ ιδιότητες κατὰ τε τὰ χρώματα καὶ τὴν προσοσφύδα.....ὁ δὲ κατὰ τὴν Θηβαΐδα, καὶ μάλιστα ὁ κατὰ τὴν Κόπτου πόλιν, οὗ ἐστὶ λεπτός, καὶ εὐανάδοτος, καὶ ταχέως πεπτικός, ὡς τοῖς πυρεταίνουσι ὀφείμενος μὴ βλάπτειν.

† Strab. viii. 1.

‡ Hist. Plant. i. 3.

we know good wine to have been made, viz. the Oasis. Now the mean temperature of Cairo is $22^{\circ} \cdot 5$ C.* The Oasis lies from 4° to 6° south. Let us take as a mean 5° . The increase of mean temperature corresponding to 5° of latitude is, as I shall presently show, for these countries, not less than $1\frac{1}{2}^{\circ}$ C. This makes the mean temperature of the Oasis 24° ; and unless we believe the climate of ancient Egypt to have been cooler than at present, we must admit that good wine may be made under that temperature.

The vine, in fact, is less impatient of heat than might be supposed from the circumstances said to attend its cultivation at Bushire. Superb grapes are grown at Cawnpore in British India in about $26\frac{1}{2}^{\circ}$ latitude, at an elevation of not more than 1000 or 1200 feet above the sea†. Humboldt informs us that excellent grapes are found at Cumana in $10\frac{1}{2}^{\circ}$ latitude with a mean temperature of $27^{\circ} \cdot 7$ C‡. It must be recollected that the heat of the Persian Gulf is excessive. The mean temperature of the month of July is 34° C., and the thermometer at noon rises even to 44° C.§, a degree of heat surpassed at few spots on the surface of our globe. To find higher temperatures than this we must go to Nubia, where Caillaud frequently observed 48° ||, and the Oasis of Mourzouk, where Ritchie and Lyon experienced even the extraordinary heat of $53^{\circ} \cdot 8$ C.; the thermometer rising daily for whole months to from 46° to 52° ¶.

To find the increment of mean temperature from Cairo to the Oasis, I have calculated two years' observations made by Browne in the Oasis of Darfâr. We know so little of the interior of Africa, that these observations possess a good deal of interest. They were made partly at Cobbé in latitude $14^{\circ} 11'$, and partly at a place called El Fasher, which seems to have been in the neighbourhood, but which I do not find on Browne's map. The hours of observation were 7 a.m. and 3 p.m., a few days only are wanting to complete two years. I find the following monthly means: —

January ...	$18^{\circ} \cdot 5$ C.	July	$30^{\circ} \cdot 6$ C.
February...	19·9	August.....	32·0
March.....	27·2	September	33·1
April	30·1	October ...	28·9
May.....	29·7	November ..	25·6
June.....	29·4	December ..	23·4

* Humboldt, *Lignes Isothermes. Mém. d'Arcueil*, vol. iii. Tableau.

† *Nouvelles Annales des Voyages*, vol. xxx. p. 317.

‡ Quoted by Schouw, *Pflanzen-Geographie*, p. 209.

§ *Voyage aux Régions Equinoxiales*, vol. xii. p. 208. 8vo edit.

|| *Voyage à Méroé et au Fleuve Blanc*.

¶ Lyon's Travels, Meteorological Register.

and hence the mean of the whole year = $27^{\circ}4$ C. Comparing this mean temperature with that of Cairo, in latitude 30° , the increase for five degrees of latitude is $1\frac{1}{2}^{\circ}$ C., as assumed before.

Probably the number $27^{\circ}4$ is rather too high, as the hours of observation selected would lead to a result erring in excess. But on the other hand it is to be remarked, that Darfûr comes within the limits of the tropical rains, which in this part of Africa reach as far as 17° north, and tend to keep down very much the heats of summer. In fact, Browne never marks the thermometer above $38^{\circ}3$ in Darfûr, while he mentions having observed $46^{\circ}7$ in the Great Oasis of Egypt. It is in those portions of the torrid African zone, where rain never or rarely falls, as in Upper Egypt, Nubia, and Fezzan, that the heat of the day is most excessive. It is true that the nocturnal radiation is also very great*, which keeps down the mean temperature of the twenty-four hours; but during certain hours the vine would be probably exposed to greater heat in the Egyptian Oasis than in Darfûr itself.

III. Cultivation of the Vine in Britain.

I think it is difficult not to feel very sceptical about any extensive cultivation of the vine in ancient times in the northernmost provinces of France, and in England. Strabo, as M. Arago has remarked, indicates the line of the Cevennes as the limit of the culture of the olive in France, which it still is; but the remainder of the sentence is, I think, equally remarkable: ἡ ἀμπελος δὲ προϊούσιν οὐ ραδίως τελεσφορεῖ†, “as one goes further into the country, the vine does not easily bring its fruit to maturity.” As to Britain, we are told expressly by Tacitus, the climate was not warm enough for the vine‡.

The scanty notices of the climate of Great Britain to be found in ancient authors agree remarkably with what we experience at the present day. Thus Cæsar says, “Loca sunt temperatiora quam in Galliâ, remissioribus frigoribus.” (*Bell. Gallic.* lib. v. c. 12.) Thus Strabo: ἐπομβροὶ δ' εἰσὶν οἱ ἀέρες μᾶλλον ἢ νιφετώδεις, ἐν δὲ ταῖς αἰθρίαις ὀμίχλη κατέχει πολὺν

* Caillaud speaks of water freezing at night on his journey between the Great and Little Oasis: at 7 a.m. the thermometer stood at $+2^{\circ}$, at noon at 19° . Capt. Lyon (p. 256.) speaks of ice half an inch in thickness in the plains of Fezzan, south of Mourzouk. After these examples, it is difficult to understand why the thin cake of ice observed by Oudney and Clapperton on their road from Bournou to Sockatoo should have excited so much surprise.

† Strab. iv. 1.

‡ Agricola, c. 12.

χρόνον. (Lib. iv. c. 5.) Thus, too, Tacitus: "Cælum crebris imbris ac nebulis fœdum; asperitas frigorum abest (fruges) tardè mitescunt, citò proveniunt: eadem utriusque rei causa, multus humor terrarum cœlique." (*Agricol.* c. 12.) And Minucius Felix, in a passage (§ 18.), for which I am indebted to the *Archæologia*, vol. iii. p. 54: "Britannia sole deficitur, sed circumfluentis maris tepore reficitur."

Several authors have quoted a passage from Vopiscus*, stating that the Emperor Probus gave permission to the Spaniards, Gauls and Britons to plant vines; which permission to the Britons, it is said, would have been a derision, had the climate been too cold for the cultivation of that plant. The first objection that arises, is that Eutropius†, an earlier historian, and Aurelius Victor, in speaking of the same circumstance, mention Gaul and Pannonia, but not Britain. Let us assume, however, the statement of Vopiscus to be correct. Why, it may be asked, was the Imperial permission necessary for this cultivation? To this it is replied, that Domitian, according to Suetonius‡, had forbidden the laying out of new vineyards in Italy, and ordered the destruction to the extent of at least one half of those existing in the provinces. Unfortunately, Suetonius adds, "nec exsequi rem perseveravit." Waving, however, this second difficulty, let us suppose, for the sake of argument, that the edict of Domitian was enforced.

Now as Tacitus, who wrote after Domitian, expressly denies Britain the vine, the olive, "ceteraque calidioribus terris oriri sueta," it follows that the vine could not have been cultivated in that island before the time of Probus, and that consequently (unless the Britons are introduced by a mistake of Vopiscus) the permission of that emperor could only have been a permission to make the experiment of the culture.

I believe the real explanation of the cause of the edict of Probus is to be found in a passage of Cicero de Republicâ, which shows that the prohibition to plant the vine in the provinces was a portion of Roman policy even during the Republic. This passage, which is remarkable in more respects than one, is as follows: "Nos vero, justissimi homines, qui Transalpinas gentes oleam et vitem serere non sinimus, quò pluris sint nostra oliveta, nostræque vineæ." (Lib. iii. c. 13.) The edict of Domitian, and various passages of ancient authors, (among whom I may mention Martial, who speaks of the wines of Marseilles, Vienne in Gaul, and Tarragona,)

* In Probo, c. 18.

† ix. c. 17. "Vineas etiam Gallos et Pannonios habere permisit."

‡ Domitian. c. 7.

prove that this system was not uniformly enforced; local exceptions were evidently made; yet enough appears to explain the value and the cause of the permission of Probus.

The first positive authority for the cultivation of the vine in Britain is Bede, who says, "*Vineas etiam quibusdam in locis germinans**." It is important here to observe the "*quibusdam in locis*." Setting aside vague traditions, the next authentic testimony is that of Domesday Book, which mentions vineyards in several places. At Rayleigh in Essex, we are told "there is one park and six arpennis of vineyard, which, if it takes well, yields twenty modii of wine." (Camden's Essex.) But the very indication of a few vineyards here and there excludes the idea of any extensive cultivation, such as takes place in really wine-growing countries. At a subsequent period, many authorities, for which I may refer to the *Archæologia*, vols. i. and iii., and Miller's Gardener's Dictionary, article *Vitis*, prove the existence of vineyards in particular spots, and generally in connexion with cathedrals or religious houses. What was the success of these attempts of the monks to make wine, "*in commodum et magnum honorem*," as an old writer says, of their respective houses, may partly be conjectured from the accounts of a vineyard at Ely given by Miller, where the sale of verjuice forms a considerable portion of the profits of the vineyard †. Only one passage has been quoted that would at all seem to imply an extensive cultivation of the vine in ancient times, and even in that the terms are too vague to allow of any positive conclusion. William of Malmesbury (quoted by Camden) boasts of the superiority of the vineyards of Gloucestershire: "*Vinearum frequentia densior, proventu uberior, sapore jucundior, vina etiam ipsa bibentium ora tristi non torquent acedine, quippe quæ parum debeant Gallicis dulcedine*." This passage does not imply much, it may be observed in passing, for the English wine in general. As to what he says about Gloucestershire, it is to be considered that he seems to be an inaccurate writer, and disposed to exaggeration. Daines Barrington has noticed that he speaks of the bore in the Severn as a daily occurrence, while it happens only at the equinox; and describes it in very exaggerated terms, as capable of sinking a ship, though in reality not formidable to a cock-boat.

* Hist. Ecclesiast., i. 1. The supposition of Daines Barrington, that in this and other passages "*vineæ*" refers to orchards of apple-trees and currant-gardens, is too improbable and unsupported to deserve serious refutation.

† In the 12th Edw. II. the wine from the vineyard at Ely sold for 1*l.* 12*s.*, the verjuice for 1*l.* 7*s.* In 9th Edw. IV. no wine, only verjuice was made.

I repeat, that to prove a change of climate it is necessary to show, not merely the existence of vineyards in a few localities, but the extensive growth of the grape for the purpose of making wine. In fact, Plot* tells us, that in the year 1685, Dr. Bathurst, President of Trinity College, made as good claret at Oxford, "in a very mean year for that purpose," as any one could wish to drink; and Pepys says, that in the reign of Charles II. very good wine was made at Walthamstow. As far then as vineyards in particular localities prove anything, the climate of Britain has been constant from the time of Bede to the year 1685. Nor has it degenerated since; for Miller gives a list of places at which wine has been made in the course of the last century; among which are Rotherhithe, Brompton, Kensington, Hammersmith, Walham Green (wine was made at this place for 30 years), Arundel, and Pain's Hill, near Cobham. The wines of many of these places are described as being equal or superior to the French wines of the second class. That made by Mr. Hamilton at Pain's Hill is said to have been fully equal to the best Champagne, and to have sold for fifty guineas a hogshead.

While on the one hand there is no sufficient testimony in favour of the growth of wine on a large scale in ancient times, there is on the other some direct testimony against it. Petrarch, according to Miller, speaks of the people in England as not drinking wine; and Daines Barrington has quoted Lord Bacon†, who says that grapes require a south wall to ripen.

All these considerations make it difficult to admit, with Arago, that the climate of Britain was warmer formerly than at the present time. This idea rests solely on the cultivation of the vine in this island; a fact which cannot be disputed, but does not, I conceive, lead to the inferences that have been drawn from it. The testimony adduced merely indicates a very local and partial cultivation of the plant; such, in fact, as numerous experiments have shown to be practicable in recent times.

R. W. R.

XVI. *Mineralogical Notices. Communicated by W. H. MILLER, Esq., Professor of Mineralogy in the University of Cambridge.*

PLUMBIFEROUS ARRAGONITE.

[From Poggendorff's *Annalen*, B. xlviii.]

A VARIETY of arragonite from Tarnowitz, found by Professor Breithaupt to have a specific gravity of 2.995,

* Camden, Staffordshire.

† Cent. v. Exp. 430. 432.

contains 2·19 per cent. of carbonate of oxide of lead, according to the analysis of Professor Kersten, and 3·859 per cent. according to the analysis of M. Böttger. Hence the proportion of carbonate of oxide of lead appears to be variable, like the proportion of carbonate of strontian in common arragonite, which, according to Stromeyer, varies from 0·5 to 4·01 per cent.

ANALYSES OF PETALITE AND SPODUMENE.

[From Poggendorff's *Annalen*, B. xlix.]

Petalite analysed by M. Robert Hagen in the laboratory of Prof. Heinrich Rose, gave the following results :

	(1.)	Oxygen.	(2.)	Oxygen.
Silica	77·812	40·423	77·067	40·036
Alumina...	17·194	8·030	18·000	8·406
Lithia.....	2·692	1·484	2·660	1·466
So da	2·302	0·588	2·273	0·581

Spodumene gave

		Oxygen.
Silica	66·136	34·357
Alumina.....	27·024	12·021
Protoxide of iron	0·321	0·098
Lithia.....	3·836	2·126
Soda	2·683	0·686

RESULTS OF ANALYSES OF POONALITE AND THULITE. BY
PROF. C. G. GMELIN.

[From Poggendorff's *Annalen*, B. xlix.]

Poonalite.

	Observed.	Oxygen.	Computed.
Silica	45·120	23·44	45·07
Alumina.....	30·446	14·22	31·33
Lime	10·197	2·86	10·43
Soda with a trace of potash	0·657	0·11	
Water	13·386	11·90	13·17

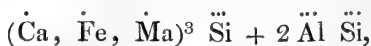
The formula to which the computed quantities refer, is



Thulite.

Silica	42·808
Alumina.....	31·144
Lime	18·726
Soda with a trace of potash	1·891
Protoxide of iron	2·288
Protoxide of manganese ...	1·635
Water.....	0·640—99·132

The above results agree with the formula



and show that the mineral is chemically the same as Epidote.

RESULTS OF ANALYSIS OF BORACITE FROM LÜNEBURG. BY

M. C. RAMMELSBURG.

[From Poggendorff's *Annalen*, vol. xlix.]

Transparent Crystals.

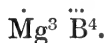
Opaque Crystals.

S. G. 2·955 at temp. 12°·5 C. S. G. 2·938 at temp. 11°·5 C.

According to
Arfvedson.*

Magnesia	30·748	31·124	30·3
Boracic acid . .	69·252	68·876	69·7

The quantities of oxygen contained in the magnesia and boracic acid are 12·03 and 47·26 respectively. Hence the composition of boracite is expressed by the formula



RESULTS OF ANALYSES OF NOSEAN, HAUYNE, LAZULITE AND

ARTIFICIAL ULTRAMARINE, PERFORMED BY M. F. VARRENT-

TRAPP IN THE LABORATORY OF PROF. HEINRICH ROSE.

[From Poggendorff's *Annalen*, vol. xlix.]

Nosean from the Lake of Laach.

		Oxygen.
Alumina	32·566	15·20
Soda	17·837	4·56
Lime	1·115	0·31
Silica	35·993	18·70
Sulphuric acid . . .	9·170	5·49
Iron	0·041	
Chlorine	0·653	
Water	1·847	99·222

Hauyne from Nieder-Mendig.

		Oxygen.
Soda	9·118	2·33
Lime	12·552	3·52
Alumina	27·415	12·80
Silica	35·012	18·28
Sulphuric acid . . .	12·602	7·23
Sulphur	0·239	
Iron	0·172	
Chlorine	0·581	
Water	0·619	98·340

[* The details of Arfvedson's analysis will be found in *Phil. Mag. First Series*, vol. lxii. p. 358.—EDIT.]

Lazulite.

Soda	9.09
Lime	3.52
Alumina	31.76
Silica	45.50
Sulphuric acid .	5.89
Sulphur	0.95
Iron	0.86
Chlorine	0.42
Water	0.12

Artificial Ultramarine.

Soda	21.476
Potash	1.752
Lime	0.021
Alumina	23.304
Silica	45.604
Sulphuric acid	3.830
Sulphur	1.688
Iron	1.063
Chlorine	a trace

98.11

TWO ANALYSES OF GMELINITE FROM GLENARM, SP. GR. 2.06
AT 12° C. BY M. CARL RAMMELSBERG.

[From Poggendorff's *Annalen*, B. xlix.]

	Oxygen.		Oxygen.	
Silica	46.398	24.10	46.564	24.19
Alumina	21.085	9.84	20.186	9.43
Lime	3.672	1.03	3.895	1.09
Soda	7.295	1.86	7.094	1.81
Potash	1.604	0.27	1.873	0.31
Water	20.412	18.14	20.412	18.14

Hence the quantities of oxygen contained in the alkaline bases, including the lime, the alumina, the water and the silica, are as the numbers 1, 3, 6, 8, as in most Chabasies. The chemical distinction between the two minerals appears to consist in the relative quantities of lime and soda contained in them, most lime being contained in Chabasie. Gmelinite forms a jelly with hydrochloric acid, Chabasie does not.

XVII. *Observations on Elæolith and Nepheline*. By THEODORE SCHEERER.*

IN the 46th volume of Poggendorff's *Annalen*, p. 291, I communicated a notice on an elæolith which is found near Brevig in Norway. I considered myself justified, from the results of the analyses made with it, in advancing a new formula for this mineral, which seems to be more in harmony with its composition than the one hitherto employed. I had

* Communicated by the Author.

then no elæoliths from other localities at my disposal with which to test the truth of my assertion, and I was consequently obliged for the moment to lay all further inquiry aside. Subsequently, however, I re-commenced these investigations, partly in Norway and then in Berlin, in the laboratory of Professor Heinrich Rose, and with the kind assistance of Mr. W. Francis, I have performed a new series of analyses of elæoliths and nephelines. The results which I have obtained, as well as some other observations, I will communicate in the present paper, and at the same time I will add for comparison the analyses of these minerals, which had been previously made by other chemists.

I. ANALYSES OF VARIOUS ELÆOLITHS.

1. *Brown Elæolith from Brevig in Norway.*—I have already communicated the composition of this elæolith in the memoir above-mentioned, but for the sake of completeness I will repeat it here. It was found, from three analyses, to consist of

	1.	2.	3.
Silica	44.59	44.48	44.30
Alumina	32.14	32.03	31.60
Peroxide of iron	0.86	1.30	1.16
Lime	0.28	0.24	0.32
Soda	15.67	15.76	} 20.45
Potash	5.10	5.24	
Water	2.05	2.06	2.10
	<hr/>	<hr/>	<hr/>
	100.69	101.11	99.93

The specific gravity of this elæolith is 2.617. It occurs in smaller or larger masses in a snow-white granular albite, and is in general so much impregnated with it that it is exceedingly difficult to obtain pure fragments for analysis. Hence on dissolving the silica in carbonate of soda some powdered albite always remains. This latter mineral being scarcely in the slightest degree attacked by acids, I do not believe that it has had any sensible effect on the analyses.

2. *Green Elæolith from Fredriksvärn in Norway.*—This species of elæolith occurs, like the following, in the well-known zirconiferous syenite of Fredriksvärn. Its specific gravity was found to be 2.61. Klaproth* was the first who undertook an analysis of this mineral, and obtained the following results:—

* Klaproth's Contributions, vol. v. p. 170.

Silica	46.50
Alumina	30.25
Peroxide of iron . . .	1.00
Lime	0.75
Potash	18.00
Water	2.00

98.50

After Klaproth, C. G. Gmelin* analysed the green elæolith, and found it to be composed of

Silica	44.19
Alumina	34.42
Peroxide of iron . . .	0.65
Lime	0.52
Soda	16.88
Potash	4.73
Magnesia and manganese .	0.69
Water	0.60

102.68

The results of two analyses performed by me, were—

	4.	5.
Silica	45.31	45.15
Alumina	32.63	32.70
Peroxide of iron . . .	0.45	0.67
Lime	0.33	0.34
Soda	15.95	15.48
Potash	5.45	5.88
Water	0.60	0.63

100.72

100.85

Between these analyses and that of Klaproth there is very little agreement. Probably either the silica which Klaproth obtained still contained alumina, or the elæolith employed by him was impregnated with foreign minerals, insoluble in acids, which must have increased the quantity of silica, as this was not tested as to its purity. This analysis becomes still more suspicious, when we consider that elæolith not only contains potash, but also soda. From 100 grains of powder Klaproth obtained 27.5 grains of chloride of an alkali, which he took for pure chloride of potassium, and calculated as such. But if we bear in mind that it was composed of the two alkalies in a proportion corresponding to the quantities of potash and soda

* Schweigger's Journal, vol. vi. p. 82.

in my analyses, we then obtain only about 11 per cent. soda and 4 per cent. potash, which gives a loss of 4·5 for the whole. Between C. G. Gmelin's analyses and mine there prevails a satisfactory coincidence; only the silica of the former should perhaps be somewhat greater, as the small quantities of it which constantly accompany the other constituents were not separated and added to the main quantity. That M. Gmelin did not redissolve the silica in carbonate of soda can scarcely have given rise to any inaccuracy, as on employing good pieces of this elæolith the silica is always found free from foreign ingredients. Likewise in the amount of alkali this analysis differs slightly from mine, but this undoubtedly arises from the method employed by him being less perfect than the one now in use for the determination of potash and soda. The *sums* of the chlorides agree on the other hand perfectly; Gmelin obtained 39·1 per cent, and I very nearly 39·0, which proves that only the relative quantities of potash and soda were somewhat inaccurately determined in Gmelin's analysis.

3. *Brown Elæolith from the same locality.*—This variety has not, as far as I am aware, been hitherto analysed, as it was always considered to be a modification of the green elæolith, which is fully confirmed by the following analyses. I am indebted to the kind aid of Mr. W. Francis for analysis 6, made in the laboratory of Professor H. Rose:—

	6.	7.
Silica	45·51	45·55
Alumina	33·53	32·00
Peroxide of iron }		1·41
Lime	0·81	trace.
Soda	15·86	16·09
Potash	4·50	5·02
Water		0·78
	<hr/> 100·21	<hr/> 100·85

The specific gravity was found to be 2·61.

4. *White Elæolith from the Ilmen Mountains in Siberia.*—The occurrence of this mineral has been described by Professor Gustav. Rose*.

The Ilmen mountains consist for the greatest part of a rock of white felspar, black uniaxal mica, and nepheline, thus exhibiting, but far more so the other minerals it contains, such as zircon, pyrochlor, &c., great similarity to the zircon-

* *Pogg. Annalen*, vol. xlvii. p. 377.

iferous syenite of Fredriksvärn. It must be considered, as it were, as a granite in which the quartz is replaced by elæolith. On account of its evident peculiarity and its large distribution, Professor G. Rose has proposed for it the name *miascite* (from the river Mias in the neighbourhood of its occurrence). Its specific gravity I found to be 2·60.

Two analyses of this mineral have recently been published by M. Bromeis*, who obtained the following results :—

Silica	42·51	42·33
Alumina	33·73	34·39
Peroxide of iron	trace	trace
Lime	0·20	0·47
Soda	14·01	16·26
Potash	6·91	5·95
Magnesia	0·77	0·45
Water	0·92	0·92
	<hr/>	<hr/>
	99·05	100·77

Mr. Francis and I have likewise examined this mineral. The result 8 is that which Mr. Francis obtained; the result 9 that which I obtained by analysis :—

	8.	9.
Silica	44·30	44·07
Alumina	33·25	33·12
Peroxide of iron	0·82	0·57
Lime	0·32	0·26
Soda	16·02	15·70
Potash	5·82	5·69
Magnesia	0·07	trace
Water	0·00	0·90
	<hr/>	<hr/>
	100·60	100·31

The results of the two last-mentioned analyses differ from those of M. Bromeis, particularly in the amount of silica. I shall subsequently return to this point.

5. *Nepheline from Monte Somma* (Vesuvius).—This mineral occurs very differently from the preceding ones, it always being found crystallized in 6-sided prisms with terminal faces perpendicular to the axis, combined with one or two hexagonal pyramids. It is situated in cavities of the dolomite, accompanied by garnet, vesuvian, anorthite, glassy felspar, &c. All these minerals present exactly the appearance of

* *Pogg. Annalen*, vol. xlviii. p. 577.

having been sublimed into the fissures and cracks of the dolomite. The specific gravity of this nepheline I found to be 2·56. It has been analysed by Arfvedson, who found its composition as follows:—

Alumina (with some iron and manganese)	33·73
Soda	20·46
Water	0·62
Silica	44·11
	<hr/>
	98·92

As it is very difficult to obtain the crystals of nepheline quite pure, without portions of the other minerals with which it is accompanied being mixed with them, I had the precaution to take a separate quantity of the crystals for each analysis without previously pulverizing the whole together. Now, if the result of the analysis was influenced by foreign minerals, this influence must at least vary in the different analyses, and consequently produce discordant results. Two analyses gave

	10.	11.
Silica	44·03	44·29
Alumina	33·28	33·04
Peroxide of iron	0·65	0·39
Lime	1·77	1·82
Soda	15·44	14·93
Potash	4·94	4·72
Water	0·21	0·21
	<hr/>	<hr/>
	100·32	99·40

The amount of water, from want of material, was determined only in one experiment. In both analyses, notwithstanding the great precaution taken in choosing the fragments of the crystals, it was impossible to prevent a quantity of foreign minerals getting into the analysis, which was, however, not decomposable by hydrochloric acid. In the first analysis this mixture amounted to about 3 per cent; in the second to about 5 per cent. As from this cause, notwithstanding the near agreement of the results, doubts might arise as to their perfect correctness, I analysed a third portion of nepheline. Through the great kindness of Prof. G. Rose I received from him for this purpose a very fine crystalline geode in pure dolomite, containing crystals perfectly free from all foreign minerals, with a portion of vesuvian which was easily removable. I found this to be composed of

12.

Silica	44·04
Alumina	34·06
Peroxide of iron	0·44
Lime	2·01
Potash	4·52
Soda	15·91
Magnesia	trace
Water	0·21

 101·19

The silica, on dissolving it in carbonate of soda, was found to be so pure that it only left 0·004 gr. insoluble matter. The amount of water was adopted from the first determination. The agreement of this analysis with the two former, sets it beyond all doubt that the foreign minerals which were present in both the former had not exercised any influence on the results. The quantity of alumina in analysis 12 seems, however, from some circumstance or other, to be stated too high, as it is about 1 per cent. greater than in the analyses 10 and 11, and the analysis gives 1 per cent. overplus. However this may be, it results from the three inquiries, 1st, that the nepheline contains a larger quantity of lime than elæolith; 2ndly, that soda and potash are contained in both. In these two points my analyses differ entirely from those of Arfvedson. He mentions, it is true, having found some lime, but that the quantity was too small for accurate determination. It may therefore be the case that all nephelines do not contain this quantity of lime, amounting to 2 per cent., if the reason of Arfvedson's not finding so considerable a quantity of lime is not to be sought for in the method he employed; viz. instead of adding oxalate of ammonia immediately to the liquid filtered from the alumina, first neutralizing it with hydrochloric acid. The point of neutralization might here be easily exceeded, and some oxalate of lime remain dissolved in the fluid. That Arfvedson found no potash, probably arises from his using for its detection a far more imperfect reagent than chloride of platinum, viz. tartaric acid.

6. *White Elæolith from Katzenbuchel in the Odenwalde.*—

This variety is found in isolated crystals in dolerite (nepheline rock), which breaks through the sandstone of the Katzenbuchel. Prof. L. Gmelin has given an analysis of it in a paper by him and Prof. Leonhardt, which I will here cite. It gave

Silica	43·36
Alumina	33·49
Peroxide of iron (with some manganese) .	1·50
Lime	0·90
Soda	13·36
Potash	7·13
Water	1·39

 101·13

I likewise have analysed a portion of this variety. It was however impossible, notwithstanding every precaution, to obtain perfectly pure crystals, as the very crystals inclosed minute parts of the rock in their interior. Their nucleus is frequently the most impure, often exhibiting a 6- or 4-sided opacity, which is environed by a more transparent frame. I found it to be composed of

	13.
Silica	43·70
Alumina	32·31
Peroxide of iron . . .	1·07
Lime	0·84
Soda	15·83
Potash	5·60
Water	1·39

 100·74

The amount of water is here adopted from Prof. Gmelin's determination, as a sufficient quantity of pure mineral was not at my disposal. The silica separated by hydrochloric acid left about 2·5 per cent. residue, insoluble in carbonate of soda, which evidently consisted of minute parts of the rock. A considerable difference between M. Gmelin's analysis and mine will be found solely in the relative quantities of potash and soda. The source of this is without doubt to be sought in the mode of determining these two alkalies employed by L. Gmelin. He weighed the heated chlorides and determined by a solution of silver the quantity of chlorine contained in them, and thence calculated the requisite quantities of chloride of potassium and chloride of sodium which must be present to give this quantity of chlorine in the mixture of the two salts. On closer consideration, it is evident that every error of weight is greatly increased by calculation, as a small defect in the quantity of chloride of silver corresponds to a considerable difference in the relative quantity of potash and soda.

II. QUANTITIES OF HYDROCHLORIC AND SULPHURIC ACID IN ELÆOLITHS.

In the preceding analyses I have omitted two constituents which well deserve a separate consideration: these are small quantities of hydrochloric and sulphuric acids which occur in these minerals. M. Bromeis first drew attention to this subject, and I have fully confirmed his observations. To test the various species of elæolith as to the quantities of these acids, I always employed distinct quantities, which, powdered finely, were decomposed by chemically pure nitric acid. The muriatic acid was then determined by a solution of silver, and the sulphuric acid by chloride of barium. M. Bromeis found in the white elæolith from the Ilmen mountains 0·04 per cent. hydrochloric acid. I obtained the following results:—

	Hydrochloric acid.	Sulphuric acid.	
Green elæolith from Arendahl . .	trace	trace	2 exp.
Brown elæolith from the same lo- cality	trace	—	
White elæolith from the Ilmen mountains	0·06	0·07	
Nepheline from Monte Somma . .	0·22	0·10	1 exp.
The same	0·05	trace	2 exp.

The first and second experiment with the nepheline differ very much, which probably arises from a quantity of those crystals being taken which were employed for the analyses 10 and 11, containing foreign minerals which, as for instance davyne, might easily increase the amount of the acids. As the quantities are so small, I will not vouch for perfect accuracy in the quantities stated*; thus much is, however, certain, that they exist in the elæoliths examined. They are not expelled by exposing the mineral to a red heat, but may be detected in the mass after heating, which is as easily decomposed by acids as before. Nor can they be extracted from the finely powdered mineral by boiling in water; it hence results that they cannot be simply combined with an earth or an alkali, but form an essential mineral combination, probably replacing a small quantity of silica. At first I suspected that the portion of silica, always remaining undissolved on decomposing the mineral with nitric acid, might possibly produce opacity with a solution of silver; but this cannot be the case, as the opacity, with different species of elæolith, under similar circumstances, differed considerably; thus, for in-

* Especially as the small quantities of the chloride of silver were always of a blackish colour, and not fusible.

stance, with the brown one from Fredriksvärn, there was scarcely a perceptible trace, while the white species from the Ilmen Mountains and the nepheline from Monte Somma, deposit a precipitate after some days' standing. Hydrochloric acid is probably a widely diffused constituent of volcanic minerals; I have, for instance, found distinct traces of it in Analcime.

III. CHEMICAL FORMULA FOR THE ELÆOLITHS.

From all the analyses above detailed, it results, to a certainty, that both elæolith, as well as nepheline, must be considered as composed of 3 atoms silica, 2 atoms alumina, and 2 atoms alkali. The alkali, however, always consists of 4 equivalents of soda to one of potash; calculated accordingly, it should be thus:

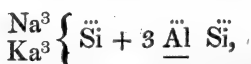
Silica	44.67
Alumina	33.12
Soda	16.12
Potash	6.09

100.00

Here, however, no notice is taken of 1°, the amount of water, 2° the amount of hydrochlorates and sulphates. If we take both into account, we find that the species of elæoliths examined contain about from 0.5 to 2.0 per cent *foreign* constituents, which must be proportionally subtracted from the *essential* constituents. An elæolith would thus be composed of

	0.5 per cent. foreign constituents.	1 per cent. foreign constituents.	2 per cent. foreign constituents.
Silica . .	44.45	44.23	43.79
Alumina	32.95	32.79	32.46
Soda . .	16.04	15.95	15.78
Potash .	6.06	6.03	5.97
	<hr/>	<hr/>	<hr/>
	99.50	99.00	99.00

But according to the view which pre-supposed in elæolith 4 at. silica, 3 at. alumina, and 3 at. alkali, adopting the formula



the three corresponding compositions should be

Silica . .	41.58	41.36	40.94
Alumina .	34.67	34.50	34.15
Soda . .	16.88	16.80	16.63
Potash .	6.37	6.34	6.28
	<hr/>	<hr/>	<hr/>
	99.50	99.00	98.00

Since all the elæoliths analysed approximate in the quantity of foreign constituents to one of these three cases, a comparison may be easily instituted between the compositions found and the corresponding calculated ones. It will then be seen that none of the analyses approach to the atomic proportion of the old formula, with the exception of M. Bromeis' two analyses; but even these still give 1 per cent silica more than is required by the old formula, although M. Bromeis undoubtedly took the greatest precaution not to find too much silica.

It might still be supposed by those who cling so fast to the sacred number of three in chemical formulæ, that some circumstance or other in the mode of analysis pursued by Mr. Francis and myself may have produced the constant supposed overplus of silica; I will therefore give a sketch of the method we employed. The mineral in powder was triturated in an agate mortar, and dried in a water bath, and was then treated with hydrochloric acid, with constant stirring, till the formation of a perfectly gelatinous mass occurred: this was left for some time, usually a day, in a warm place, and then in some analyses evaporated to dryness; and in others, still containing acid, treated immediately with water. The mass evaporated to dryness was again moistened with acid, and after an hour water was added to it and it was filtered. The silica, after incandescence and weighing, was tested as to its purity by boiling with carbonate of soda. Small quantities of silica were subsequently separated from the alumina, peroxide of iron, and the mixture of chloride of potassium, and chloride of sodium, and were added to the chief quantity. With this method there are four points of doubt, in which it is uncertain whether the amount of silica is influenced. 1. It may be questioned whether, when the mass of decomposed mineral, evaporated to dryness, is moistened with hydrochloric acid, and left to stand in this state for an hour or so, all the hydrochlorate of alumina, which has become basic, is re-dissolved. That this is, however, the case, is evident from there being no perceptible difference in the amount of silica, whether all the acid is expelled or the gelatinous mass, containing immediately acid treated with water. 2. On boiling the silica which had been heated to redness with a concentrated solution of carbonate of soda, there was always left an insoluble residue. Two cases must here be distinguished. This residue either consists of light flocks having the appearance of silica, or of these and a heavier powder of a sandy nature, and which is evidently some foreign mineral not decomposed by the hydrochloric acid; it must, therefore, in per-

fect accuracy, be subtracted from the quantity employed for the analyses. But the flocks M. Bromeis considers to be alumina, which was still retained in the silica, and was not dissolved on boiling in carbonate of soda. This, however, is not the case; for if these flocks are filtered and dried, they form a powder which, before the blow-pipe with soda, gives a perfectly clear glass. To be more surely convinced of the nature of this residue, I decomposed 10 gr. of the white, and 10 gr. of the green elæolith with hydrochloric acid. In both cases I obtained from the silica sufficient quantities of this residue to be able to examine it more accurately. I found it to contain above $\frac{2}{3}$ silica, and only a little alumina, and a trace of the peroxide of iron; it cannot, therefore, be added to the alumina without committing an error. 3. M. Bromeis conceives that alumina may still be contained in the silica, separated from the heated alumina, by long digestion, in hydrochloric acid. The quantity of it can, however, be but extremely slight, as this residue acted before the blow-pipe as pure silica. If, indeed, which is possible, it should still contain a small quantity of alumina, it is certainly balanced by the trace of silica, which is re-dissolved on boiling with hydrochloric acid; for if the alumina and peroxide of iron, dissolved in hydrochloric acid, be again precipitated by ammoniac, the precipitate again heated and dissolved still gives traces of silica*. 4. Of what does the residue consist which remains on dissolving the chlorides of potassium and sodium? In the first analyses I always added it to the silica; but in so doing I committed an error, as I subsequently found it to consist of alumina, silica, lime, and at times magnesia. The first analyses give, therefore, according to my formula, too much silica. I was deceived by the insolubility of this residue in acids, but afterwards found that it did not give a glass on being treated with soda before the blow-pipe.

In the later analyses I have attended to all these points. The powdered mineral was decomposed with dilute hydrochloric acid, which has the advantage, that the elæolith dissolves in it to a clear fluid, and the particles of foreign undecomposed mineral may easily be detected.

After all the facts quoted, it seems to me fully established that elæolith and nepheline contain 3 at. silica, 2 at. alumina,

* That a portion of the silica is re-dissolved, may be explained in the following manner. Silica which, after incandescence, is insoluble in acid, becomes soluble when fused with an alkali. But even this is not requisite; many other of the stronger bases have the property of rendering it *partly* soluble.

and 2 at. soda and potash. These elements can in no way be more simply combined than in the formula $\begin{smallmatrix} \text{Na}^2 \\ \text{K}^2 \end{smallmatrix} \left\{ \begin{smallmatrix} \text{Si} \\ \text{Si} \end{smallmatrix} + 2 \text{Al Si} \right\}$; or, if regard be had to the proportions of potash and soda ($\text{K}^2 \text{Si} + 2 \text{Al Si} + 4 (\text{Na}^2 \text{Si} + 2 \text{Al Si})$). That in these formulæ the alkali (semi-silicate) is combined with more silica than the alumina ($\frac{1}{3}$ silicate) cannot appear objectionable; it is, indeed, plain that this must always happen, if, on the formation of a mineral, there is not sufficient silica present fully to saturate all the bases; these will then divide the quantity of silica among themselves according to their affinity. Thus, for instance, feldspars, as a constituent part of granite, is always in contact with superfluous silica; potash and alumina which it contains are perfectly saturated with it. But the elæoliths, whether in syenite, miascite, nepheline-rock, albite, &c., do not occur accompanied by *free* quartz. There was, therefore, a want of silica at their formation, and the potash accordingly, on account of its great affinity, took up a greater portion of it than the alumina.

The proportion of the oxygen in the alkali, relative to that of the silica, as 2:3 is, moreover, not wholly new in mineralogical formulæ; for, according to the investigations of Walmstedt*, repeated by Regnault†, prehnite should have the formula $\text{Ca}^2 \text{Si} + \text{Al Si} + \text{H}$; but very probably a similar relation occurs in many other minerals where the analysis does not agree well with the formula. I will not here mention my suspicions, but only give one instance, which speaks evidently in favour of my opinion. This is, the cancrinite, recently analysed by Professor Gustav Rose‡. Two analyses gave

Silica . . .	40·59	40·26
Alumina . . .	28·29	28·24
Soda . . .	17·38	17·66
Potash . . .	0·57	0·82
Lime . . .	7·06	6·34
Carbonic acid	6·38	6·38
	<hr/>	<hr/>
	100·27	99·70

Professor G. Rose advances accordingly the following formula, of which, however, he himself adds that it does not agree well: $\text{Na}^3 \text{Si} + 3 \text{Al Si} + \text{Ca C}$.

* Berzelius, Jahresbericht, No. V. p. 217.

† Ann. des Mines, Ser. III. T. xiv. p. 154.

‡ See the above-mentioned Memoir on the Ilmengebirge.

The mineral might therefore be regarded as consisting of 1 atom soda elæolith (according to the *older* formula), and of 1 atom calcareous spar, and should have the following composition :

Silica	38·23
Alumina	31·89
Soda	19·41
Lime	5·89
Carbonic acid . . .	4·58

100·00

But if my formula be adopted as the correct one for elæolith, cancrinite would be expressed by the formula $\text{Na}^2 \ddot{\text{Si}} + 2 \ddot{\text{Al}} \ddot{\text{Si}} + \dot{\text{Ca}} \ddot{\text{C}}$, and calculated accordingly, should have the composition :

Silica	39·11
Alumina	28·98
Soda	17·65
Lime	8·03
Carbonic acid . . .	6·23

100·00

A more satisfactory agreement of the formula with the composition found can scarcely be expected in analysis. They appear merely to differ in the formula giving the amount of lime about 1 per cent. higher ; but this difference disappears almost entirely if we admit that the potash replaces a portion of the lime. That lime and potash replace one another is evident from the analyses 10, 11, and 12 of nepheline. This latter is, indeed, only essentially different from the other elæoliths by the greater quantity of lime it contains ; but it seems that principally the amount of potash has been diminished by its occurrence, while the soda does not at all differ in proportion. Cancrinite, therefore, actually contains 1 at. of nepheline of a composition, such as was formerly adopted for the Vesuvian nepheline, but which has hitherto not been found in an isolated mineral.

On closer consideration, it will be found that the cancrinite affords even a better test for the correctness of my formula than the elæolith itself. The atomic value of the elæolith, according to the older formula (if we add 4 $\ddot{\text{Si}}$, 3 $\ddot{\text{Al}}$, 2·4 $\dot{\text{Na}}$, and 0·6 $\dot{\text{Ka}}$ together), equals 5529, according to the new one (if 3 $\ddot{\text{Si}}$, 2 $\ddot{\text{Al}}$, 1·6 $\dot{\text{Na}}$, and 0·4 $\dot{\text{Ka}}$ are added) = 3877.

Now since the weight of 1 at. of carbonate of lime is 632, it results that considerable differences must originate by these different atomic values on the per centage calculation of the constituents of a combination of both atoms. In fact, were the atomic weight of elæolith = 5529 the cancrinite ought not to contain more than about 10 per cent. carbonate of lime; while, according to my formula, it should contain 14 per cent. But the latter number is likewise confirmed by the two analyses of G. Rose.

IV. COLOUR OF THE ELÆOLITHS.

Only two of the analysed varieties of this mineral possess colour, those from Fredricksvärn, the one green, the other brown. When both are finely powdered and decomposed with concentrated hydrochloric acid, the silica separated has the same colour, only in a less degree. This is especially evident in the green elæolith, which is more intensively coloured than the brown, and with which I therefore chiefly performed experiments. Even when the concentrated hydrochloric acid is evaporated, and the dry mass again moistened with acid, and treated with water, the filtered silica still retains its colour, which, however, immediately disappears on heating the silica. It is further destroyed when the silica is heated with nitric acid, or the mineral decomposed by fuming nitric acid. This latter action sufficiently proves the colour to be of organic origin. But the colouring substance must certainly be of a peculiar nature, as it withstands the action of fuming, nay, even chloriferous hydrochloric acid.

The following are the main results afforded by these examinations of the elæoliths. 1. The formula for elæolith

and nepheline must be altered to $\begin{matrix} \text{Na}^2 \\ \text{K}^2 \end{matrix} \left\{ \begin{matrix} \text{Si} + 2 \text{Al Si} \end{matrix} \right.$. The

atomic relation of soda and potash is as 4 : 1. Both minerals are perfectly identical, the latter being merely characterized by a somewhat greater amount of lime.

2. Elæoliths, from the most varied localities, exhibit traces of hydrochloric and sulphuric acids, especially of the former.

3. The amount of water in elæoliths varies considerably, and must be considered as accidental.

It is probably only hygrometric, and is perhaps prevented from evaporating at 80° R. by a force similar to that which several porous bodies exercise on gaseous substances.

4. The colour of some elæoliths is of organic origin.

5. The specific gravity of the elæoliths is exceedingly near to 2.6.

XVIII. *On the Theoretical Constitution of the Compounds of Ammonia.* By ROBERT KANE, M.D., M.R.I.A.

IN the course of the investigations to which I have subjected the various classes of compounds that ammonia is capable of forming, it has been my lot to submit to the consideration of chemists a great number of theoretical views to which I had been led by my experimental results, and by which I conceived that the mutual connexion of the different classes of ammoniacal compounds could be explained, and their origin and properties accounted for more satisfactorily than it was possible to do by means of the ideas that had been previously received in science. In advancing this new theory of the nature of ammonia and its compounds, I was not so sanguine as to expect that our ideas of a department of chemistry so complex and so important could be immediately or easily modified, or that the adoption of my views could take place without much conflicting reasoning and discussion. In this respect I have had cause to be very much gratified. All principles that can be considered as really vital to my theory have been adopted by the most eminent chemical philosophers, and in place of being dissatisfied that in the collateral parts of the theory some portions have been thought not positively proved, and which have hence been criticized and left for the time aside by Graham and by Rose, I was at once surprised and pleased to see how little had appeared in the eyes of these acute-minded chemists unfit for being at once adopted into science.

I believe, however, that even in those portions of the theory to which Graham and Rose have not acceded, some of the difficulties arise from a want of clearness and detail in the description of my views, into which error I fell from being too anxious to avoid prolixity. As also since that period some additional evidence has been obtained which corroborates my opinions, I shall now advert to those points which are yet debated, and perhaps place them in a clearer point of view than had been done in my former paper. So far as regards the action of ammonia without water, all my ideas have been adopted; but in the relation of the ammonia and water in the common ammoniacal salts, where the ammonium theory of Berzelius comes into question, the evidence for my theory has not appeared so perfect. In fact, in order to see the true relation of the Berzelian theory to mine, it is necessary to contemplate the common salts of ammonia under two different points of view,—1st, *their position as alkaline salts*, and 2nd, *their po-*

sition as compound bodies, without reference to any other circumstance. In the first the proper theory of the salts of ammonia is that of Berzelius, but for the second purpose it is necessary to adopt the ideas on which mine is founded.

For in fact the question is, What is sal-ammoniac? Its most striking philosophical character is its equivalence to chloride of potassium. It has the same crystalline form. It enters into combination subject to the same laws. They are two bodies formed decidedly upon the same plan. But chloride of potassium contains only two elements, while sal-ammoniac contains three. There is one common to both. The residual elements are equivalent, and Cl K and Cl N H_4 , as well as K and N H_4 , are bodies which correspond to each other. Ammonium when isolated, as it has, in the amalgam, all but been, appears to possess the properties of an alkaline metal; it markedly resembles potassium. That is the Berzelian theory, in which to the full I believe as well as Berzelius. The equivalency of sal-ammoniac and chloride of potassium is a fact, and the equivalency of the K in the one and of the N H_4 in the other, is the natural inference from it. When therefore the equivalency of the ammoniacal and potash salts is under question, the ammonium theory is correctly used: it is not ammonia, it is not amidide of hydrogen which replaces potash, but it is to be called oxide of ammonium in comparison.

But if we for a moment cease to consider the relation of the ammoniacal and the potash compounds, and taking sal-ammoniac by itself, proceed to examine what light can be derived from other sources towards illustrating its internal constitution, the question presents itself, can we believe the ammonium to be ready-formed in sal-ammoniac? Can we consider the ammonium, which in the amalgam gives up its hydrogen spontaneously, to retain it so strongly when in contact with iodine or chlorine, and to enter into combination only as a single and perfect group? The answer to this question, in the framing of which all the classes of ammonia compounds require to be taken into account, led me to the development of my theory.

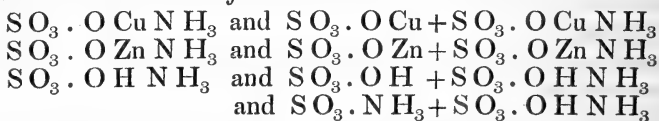
The combinations of ammonia with the anhydrous chlorides of copper, zinc, and mercury, resemble in all essential characters sal-ammoniac, and moreover, like it, those which are volatile or soluble without decomposition are found to belong to the regular system of crystallization. They further combine with the metallic chlorides of the magnesian class to form double chlorides. Thus there are

1. $\text{Cl H}_4 \text{N}$ and $\text{Cl Cu} + \text{Cl H}_4 \text{N}$.
2. Cl Cu N H_3 and $\text{Cl Cu} + \text{Cl Cu H}_3 \text{N}$.
3. Cl Zn N H_3 and $\text{Cl Zn} + \text{Cl Zn H}_3 \text{N}$.
4. Cl Hg N H_3 and $\text{Cl Hg} + \text{Cl Hg H}_3 \text{N}$.

The completeness of the analogy thus indicated is acknowledged by Graham, who proposes to extend the Berzelian theory so as to include these cases. He assumes, that in the compound radical ammonium the hydrogen may be replaced by a metal, and thus a cuprammonium $\text{N H}_3 \text{Cu}$, a zincammonium $\text{N H}_3 \text{Zn}$, a hydrargammonium $\text{N H}_3 \text{Hg}$, may be capable of individual existence. If Hg and H are replaceable, then $\text{Hg}_2 \text{H}_2 \text{N}$ is equivalent to $\text{H}_4 \text{N}$, and thus he agrees with me that the type of sal-ammoniac and white precipitate ($\text{Cl H} \cdot \text{H Ad}$ and $\text{Cl Hg} + \text{Hg Ad}$ on my theory) is the same.

But why have we not $\text{Cl Hg}_3 \text{H N}$, or $\text{Cl Cu}_3 \text{H N}$, or $\text{Cl Hg}_4 \text{N}$ and $\text{Cl Zn}_4 \text{N}$, taking their place among these bodies generated by ammonia? For $\text{Cu}_4 \text{N}$ or $\text{Zn}_4 \text{N}$ would also be equivalent to ammonium. The replacement stops when there remain yet two equivalents of hydrogen to the nitrogen; and it is only by a temperature such as destroys completely the constitution of these bodies that a metallic azoturet can be produced.

The compounds containing oxygen acids are precisely similar to those chlorides just described. The bodies



are so obviously similar in constitution, that the one explanation of their internal structure must be admitted.

There exist thus two sorts of compounds, which are ordinary ammoniacal salts with metallic oxide in place of water, or in the words of the theory of Graham, that contain ammonium in which hydrogen is replaced by a metal; those as $\text{Cl} \cdot \text{Hg N H}_3$, in which one equivalent, and those as $\text{Cl} \cdot \text{Hg N H}_2 \text{Hg}$, in which two equivalents have been thus replaced. The substitution stopping there shows that N H_2 is fixed, and thus that even if these various sorts of ammonium be admitted, the amidogene must be considered as pre-existing in them; and as Graham admits my formula for ammonium $\text{N H}_4 = \text{Ad} \left| \begin{array}{c} \text{H} \\ \text{H} \end{array} \right|$,

so his metallic ammoniums become



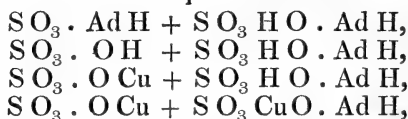
Under this form the ammonium theory is capable of being

extended so as to embrace a large class of the ammoniacal compounds newly discovered,—but can it embrace all? When calomel is treated by water of ammonia there is formed the body, $\text{Hg}_2\text{Cl} + \text{Hg}_2\text{NH}_2$. If the ammonium theory be applied to this body, are we to make a new compound radical, and say it is $\text{Cl} + \text{N H}_2\text{Hg}_4$, and to say that the corresponding body $\text{SO}_3 \cdot \text{Hg}_2\text{O} + \text{Hg}_2\text{Ad}$ is $\text{SO}_3 \cdot \text{O N H}_2\text{Hg}_4$? Here, then, is no parallel whatever; these bodies lie altogether out of the possibility of replacement connecting them with ordinary ammonium; and it would be far too violent a supposition to assume the existence of a body, N H_6 , in order to support the disputed existence of a more likely body, N H_4 . I consider this example as being fatal to Graham's view. Sublimate Cl Hg gives white precipitate $\text{Cl Hg} \cdot \text{Hg Ad}$, calomel Cl Hg_2 gives black precipitate $\text{Cl Hg}_2 \cdot \text{Hg}_2\text{Ad}$; relations so simple, so natural, that it should require very strong reasons indeed to prove that they are not those consonant to truth.

If the theory of metallic ammoniums were adopted, it would be only just to give to it its proper form. A sulphate of ammonia perfectly isomorphous with sulphate of potash contains $\text{SO}_3 \cdot \text{N H}_3 + 2\text{H O}$. The replacing element of the potassium is therefore ($\text{N H}_5\text{O}$); there is no doubt of this; it is one of Mitscherlich's best-established determinations; (N H_4) and ($\text{N H}_5\text{O}$) are equally isomorphous with potassium. Hence as sublimate treated in the cold with ammonia gives Cl N H_4 and $\text{Cl} \cdot \text{N H}_2\text{Hg}_2$, so in boiling water we get $\text{Cl} + (\text{N H}_2\text{Hg}_4\text{O}_2)$ equally equivalent, and a sort of complex ammonium. The ammonia turbeth is thus: $\text{SO}_3 + \text{O}(\text{N H}_2\text{Hg}_4\text{O}_2)$; there is $\text{N O}_5 + \text{O}(\text{N H}_2\text{Hg}_4\text{O}_2)$ and $\text{I}(\text{N H}_2\text{Hg}_4\text{O}_2)$, and so on. Now the correspondence of the common oxychloride of mercury to these bodies has been proved by Ullgren, and it should therefore be looked upon as a chloride of a compound radical $\text{Cl} + (\text{Hg}_4\text{O}_3)$. This is truly the principle involved in Graham's idea of compound ammoniums; for no matter where we begin, we find the chain by which the common salts of ammonia and the common basic salts are connected so perfect, that whatever principle we apply to one includes the others. Already, two years ago (May, 1838, *Annalen der Pharmacie*), I started the question, Were basic salts salts of compound radicals? and Liebig, in adapting a theory to the salts of platina discovered by Gros, approached nearly to the embodying of the same idea; but further examination showed me that it is one which is at present quite unfit for science, the even partial adoption of which would throw into confusion the most positive and simplest systematic arrangements that chemistry possesses, and hence do much harm and no good. Having so

far considered the degree of weight which should be attached to the extension of the ammonium theory proposed by Graham, I shall now pass to the objections which have appeared to Rose to lie against some portions of my theory.

Admitting the consistency and completeness of the arrangement which the compounds of ammonia with the dry oxygen acids and with the hydrogen acids assume according to my views, the illustrious analyst of Berlin yet considers that the assimilation of the hydrated ammonia salts of oxygen acids to those salts of the same acids which contain two equivalents of base is forced and unnatural; and he says that in place of attending to the great fact of the isomorphism of ammonium and potassium, I have neglected and suppressed that fact. This I by no means did; but this isomorphism was not the only thing to be taken into account. In fact, when all things were considered, the argument about the isomorphism of the two alkalies is of most force on the other side, and my opinion is that on the side of ammonia we have outflanked, as it were, the line of metallic bases, and that the constitution of ammonium, subamidide of hydrogen, is that which we shall hereafter find the alkaline metals to possess. The masterly researches of Rose himself on the sulphates and carbonates of ammonia are, as I believe, remarkably in favour of my view. In the carbonates of ammonia $\text{C O}_2 \cdot \text{N H}_3 + \text{C O}_2 \cdot \text{N H}_3 \cdot \text{H O}$ and $\text{C O}_2 \cdot \text{H O} + \text{C O}_2 \cdot \text{H O} \cdot \text{N H}_3$ what complete evidence do we obtain of the identity of type of $\text{N H}_3 = \text{Ad H}$ and H O ? In like manner if we look upon the series



we are driven, in order to avoid considering the recognised ammonium salt as a salt with two equivalents of base, to the adoption of the views of metallic ammoniums already sufficiently refuted.

On my theory the only hypothetic assumption is the existence of amidogen. The subsequent principles adopted that

1. Ammonia N H_3 is $\text{N H}_2 \cdot \text{H} = \text{Ad} + \text{H}$,
2. Sal-ammoniac $\text{Cl H} \cdot \text{N H}_3$ is $\text{Cl H} + \text{H Ad}$,
3. White precipitate $\text{Cl Hg}_2 \text{N H}_2$ is $\text{Cl Hg} + \text{Hg Ad}$,
4. Sulphate of ammonia $\left. \begin{array}{l} \\ \end{array} \right\} \text{S O}_3 \cdot \text{H O N H}_3$ is $\text{S O}_3 \cdot \text{O H} + \text{H Ad}$,
5. The black substance $\text{Cl Hg}_4 \text{N H}_2$ is $\text{Cl Hg}_2 + \text{Hg}_2 \text{Ad}$,
6. Ammonium if ever isolated N H_4 is $\text{H}_2 \text{Ad}$,

are all experimental and necessary results; there is nothing

hypothetical about them; everything follows from experiment.

Now the existence of amidogene is one of the best-established and most universally received hypotheses in chemistry; moreover, it is adopted also by Graham and by Rose. But how many more hypotheses must they adopt for the theory of ammonium! I shall only count up a very few, or better, state that the number of hypothetical bodies necessary for the complete ammonium theory would be equal to the number of possible metallic amidides, therefore equal to the number of metallic chlorides and oxides at present known; and this without at all touching on the theory of basic salts, which, as I have shown, is a necessary consequence of the theory of complex ammoniums.

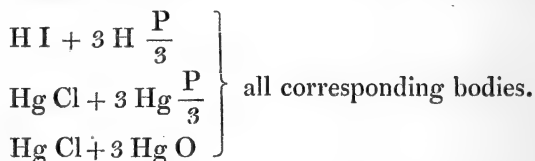
If general and simple laws can be obtained by the introduction of an hypothesis, and according as experimental research proceeds, the new facts gained are found to regulate themselves according to it, a real and important service is conferred upon science by him to whom we are indebted for it. But when a theory must change its shape and make a new assumption for each new fact discovered, as soon as the direct tendency of the theory is at an angle with that of research, and it must tack from side to side to keep the course of discovery in its line, its day has passed; and notwithstanding the services rendered to chemical theory by the hypothesis of ammonium, it is now, as I conceive, incapable of retaining its old position. Its great utility was in fixing attention on the relations of the ammoniacal and potash salts; but for explaining the immensely extended classes of compounds which ammonia is now known to form, it is insufficient.

In a paper lately published by Mitscherlich, he describes a compound of chloride and azoturet of mercury $2 \text{ Hg Cl} + \text{N Hg}_3$, and he makes an observation which, as connected with the present subject, I shall here notice. He says that the equivalent of white precipitate is not $\text{Cl Hg} + \text{Hg Ad}$, but three times that, because it requires $3 (\text{Cl Hg} + \text{Hg Ad})$ to give $\text{Cl Hg} + 2 \text{ N H}_3$ and $2 \text{ Cl Hg} + \text{N Hg}_3$: he says that also amide of potassium K N H_2 should be taken 3 K Ad , because it gives 2 N H_3 and $\text{K}_3 \text{ N}$. Now this appears to me to be a very irrational method, for then the body Cl Hg N H_3 , the simple formula of which Mitcherlitch admits, should be $8 (\text{Cl Hg N H}_3)$, because it gives $2 \text{ Cl Hg} + 3 \text{ Cl Hg}_2 + \text{N} + 4 \text{ N H}_3 + 3 \text{ Cl N H}_4$. The hydrate of phosphorous acid should be $4 \text{ P O}_3 + 3 \text{ H O}$, because it gives P H_3 and 3 P O_5 ; and a crowd of other examples might be brought forward. The equivalent formula of a body cannot be fixed thus from a single action or property.

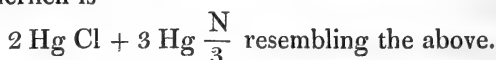
The formula of sulphate of ammonia is not $4 (S O_3 \cdot N H_4 O)$, because by heat it gives $2 (S O_2 \cdot N H_4 O) + 2 S O_2 + 2 N$, but every property must be taken into account, and our idea of the body derived from a careful induction, based on a study of all the facts known of it and of its congeners.

The red substance described by Mitscherlich resembles, in fact, those obtained by Rose with sublimate and phosphuretted hydrogen, and the bodies I have myself described, containing arsenic. In fact, as Laurent and Bineau have noticed for azote, and I myself for arsenic and phosphorus, these substances replace oxygen or amidogene in the proportion of one-third of their ordinary equivalent. Phosphuretted hydrogen

$P H_3$ does not resemble ammonia $N H_3 = Ad H$, but $\frac{P}{3} \cdot H$ resembles $Ad H$ or $H O$. The compound of iodide and phosphuret of hydrogen resembles not the hydriodate of ammonia, which is $I H + \frac{1}{3} Ad$, but the oxy-chloride of mercury, and the compound of chloride and phosphuret of mercury is similar. There are thus



Now the ammonia compounds when decomposed by heat pass into this class, in one or two cases the action being sufficiently violent spontaneously to effect it, and the substance of Mitscherlich is



I discovered this body myself when analysing white precipitate; but as I did not wish to stray from the direct discussion of the amidides, I did not publish anything about it at the time. I formed also some others of the same class, which, as soon as I can obtain leisure I will complete the examination of, and give the details of their history.

XIX. On the Composition of Inulin. By Mr. E. A. PARNELL*.

THIS substance, which was first discovered by V. Rose in the root of *Inula Helenium*, in 1804, has since been found by Payen and other chemists in several other roots, as

* Communicated by Professor Graham,

Angelica Archangelica, *Colchicum autumnale*, *Helianthus tuberosus*, &c., and in a few lichens, as *L. fraxineus* and *L. fastigiatus*. Although known to be closely related to starch, and interesting as a probable member of the starch family (especially in being converted into gum and starch sugar by the action of dilute acids), no examination of its composition has, as far as I am aware, been made. To supply this deficiency, I have performed a few analytical experiments on inulin and its compounds, the results of which form the subject of the present communication.

The inulin analysed was prepared from the root of the dahlia, as follows: the moist root, with the skin previously removed, was sliced, macerated, and washed with cold water. This was boiled in five parts of water for about an hour and a half, and filtered. The solution was nearly colourless, and quite neutral to test paper*. It was then evaporated until a pellicle appeared on the surface, and on setting aside to cool a large quantity of inulin was deposited in the form of a white pulverulent precipitate. This was collected on a calico filter, and washed with cold water until all the salts present were removed. It was then perfectly tasteless. Dried at a gentle heat, it became gummy, transparent, and easily pulverised; very soluble in hot, but sparingly in cold water. The liquid filtered from the first deposit of inulin gave an additional quantity on evaporation, which was obtained pure by washing, resolution, and evaporation.

That used in the two first analyses was prepared by adding alcohol to a strong aqueous solution; on standing the inulin was deposited perfectly pure. That used in the third analysis was made without alcohol.

1st. — 6·70 grains gave 10·65 carbonic acid, and 3·824 water.
 2nd. — 6·82 10·87 3·960 ...
 3rd. — 7·38 11·72 4·260 ...

	1.	2.	3.	Mean.
Carbon . . .	43·95	44·07	43·90	43·97
Hydrogen .	6·34	6·45	6·41	6·40
Oxygen . . .	49·71	49·48	49·69	49·63
	<hr/> 100·00	<hr/> 100·00	<hr/> 100·00	<hr/> 100·00

This nearly approaches the formula $C_{24} H_{21} O_{21}$; thus

* Payen directs chalk to be added, a free acid being present.

24 Carbon	1834·5	43·71
21 Hydrogen	262·0	6·25
21 Oxygen	2100·0	50·04
	<hr/>	<hr/>
1 Equivalent inulin	4196·5	100·00

The analogy of this formula (the lowest whole numbers which can be given) to gum, starch, grape and cane sugars, will at once be apparent; each of these containing the molecular group, 24 carbon, united to hydrogen and oxygen, in the proportions to form water. But in the whole of these (with one exception, the compound of chloride of barium and cane sugar, lately examined by Peligot, $C_{24} H_{21} O_{21} + Ba Cl$, or $C_{24} H_{18} O_{18} + 3 H O, Ba Cl$), the number of atoms admitting of division, the equivalent becomes doubtful. In crystallized cane sugar, for instance, should the formula be $C_{24} H_{22} O_{22}$, or $C_{12} H_{11} O_{11}$? While in the case of inulin this division is impossible.

To satisfy myself as to the constitution of this substance, I endeavoured to combine it with metallic oxides, but the only one attended with success was oxide of lead. Neither acetate nor subacetate of lead precipitates the aqueous solution of inulin; but on mixing its solution with the ammoniacal acetate of lead, or on adding ammonia to the mixed solutions of inulin and acetate of lead, a very bulky white precipitate occurs, which is a hydrated inulate of lead. On heating and stirring it partly dissolves, and the remainder aggregates to a heavy tenacious mass, which also dissolves if sufficient water be present. It is readily obtained pure by washing the precipitate as first produced in the cold with cold water, it being impossible to wash clean after it has aggregated. But there exist two of these inulates, possessing precisely the same physical appearances, and only distinguishable by ultimate analysis. I have procured both by the above process, and have in vain sought for the peculiar circumstances of the formation of each. Neither temperature, order of mixing the ingredients, relative proportions, nor strength of solutions, influences it in the slightest degree. The one most commonly formed possesses the composition $C_{24} H_{21} O_{21} + 5 Pb O$; the other $C_{24} H_{18} O_{18} + 3 Pb O$, both being dried at 212° .

I may remark, that I have always obtained either one or the other of these compounds, and never an indefinite mixture of both.

The mean of several analyses of one is, carbon 16·65, hy-

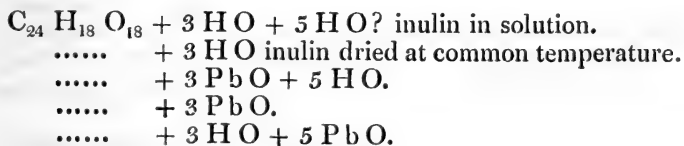
drogen 2·44 per cent., with 62·43 per cent. of oxide of lead; while the other contains carbon 22·46, hydrogen 2·94, with 51·23 per cent. of oxide of lead. The following formulæ are the nearest which can be given to these numbers:

		Calculated.	Found.
24 Carbon	1834·4	16·42	16·65
21 Hydrogen	262·0	2·35	2·44
21 Oxygen	2100·0	18·81	18·48
5 Oxide of lead. . .	6972·5	62·42	62·43
	<hr/>	<hr/>	<hr/>
	11168·9	100·00	100·00

and

		Calculated.	Found.
24 Carbon	1834·4	22·81	22·46
18 Hydrogen	224·6	2·79	2·94
18 Oxygen	1800·0	22·38	23·37
3 Oxide of lead. . .	4183·5	52·02	51·23
	<hr/>	<hr/>	<hr/>
	8042·5	100·00	100·00

As first precipitated in the cold, these inulates are hydrated; they retain this water if dried in the air, give it off slowly by being kept over sulphuric acid, and readily to air at 212°. The one which contains 5 PbO gave only 2·00 per cent., the other gave 7·00. This last, with allowance for hygrometric moisture, approaches 6·4, which is the calculated number for five equivalents, making the formula $C_{24}H_{18}O_{18} + 3PbO + 5HO$. Thus, it would appear, that anhydrous inuline is $C_{24}H_{18}O_{18}$ (isomeric with anhydrous cane sugar), which, in the case of inulin dried at common temperatures, is combined with 3 HO. With this water it does not part without decomposition. In the two lead compounds inulin is combined, in one with 3 HO + 5 PbO, and in the other with 3 PbO + 5 HO. It is also possible that inulin in solution may be combined with eight equivalents of water, thus making the series



That the group of three atoms of water is more strongly attached than the group of five atoms, is proved, first, by dry inulin containing three, and not five atoms of water, in combination with $C_{24}H_{18}O_{18}$; and secondly, by the compound $C_{24}H_{21}O_{21} + 5PbO$ giving off no water without destruction.

tion of the remainder, while the other lead compound loses five equivalents of water at 200° .

The constitution of these substances may be viewed in a somewhat different light. Water, existing as such, combined with any substance allied to starch, either basic or constitutional, as supposed above, is never known to be *so intimately combined* with the substance as to be incapable of displacement by oxide of lead, especially under such a favourable circumstance as the presence of free ammonia. When substitution appears impossible, it is therefore reasonable to conclude that the water, if it exists at all, is neither basic nor constitutional, but is essential to the substance itself.

The remarkable circumstance of inulin giving off at one time three equivalents of water, and not at another, under apparently the same circumstances, suggests the idea that two inulines exist, having the same composition, but one containing water capable of displacement, which the other does not. This substance has, indeed, been obtained by chemists in two different physical states, gummy and pulverulent (although I have never obtained but the former dry); and I would suggest the probability of these having the constitutions above-mentioned, that is, that one is $C_{24}H_{21}O_{21}$, and the other $C_{24}H_{18}O_{18} + 3HO$; a feeble but unknown cause being sufficient to convert one into the other.

This is not a singular instance of a double constitution. In fact, the only satisfactory means of accounting for dimorphism, is by supposing a difference in the constitution of the same substance at one time to that which it possesses at another, the ultimate composition continuing the same. In diabetic sugar, also, we have reason to suspect a double constitution, the descriptions of this substance by various authors being, as is well known, highly discordant. Some identify it with starch sugar; others, on the contrary, describe it as a peculiar substance. This is an interesting and very extensive inquiry, and would probably throw light on some of the cases of isomerism still unexplained.

University College, London,
May 16, 1840.

XX. *On certain Effects of Temperature.* By C. T. COATHUPE, Esq.

To the Editors of the Philosophical Magazine and Journal.
GENTLEMEN,

HAVING, from the nature of my occupations, an excellent laboratory for observing the effects of temperature, I beg

to offer you some experiments illustrative of some of these effects.

A modern glass-house is generally a cone built of brick, having its interior diameter at the base, varying from 58 to 60 feet, and its perpendicular height varying from 90 to 100 feet. The upper aperture through which the smoke ascends, varies from 9 feet 6 inches to 10 feet in diameter. This cone terminates at its base in substantial pillars of brick about 3 feet square, following the exterior inclination of the surface of the cone, and united above by arches which spring from pillar to pillar, and below by inverted arches beneath the ground.

Around the centre of the interior floor of this cone the furnace is erected; and around the exterior of the pillars which support the main body of the cone, the glass-house is extended by shed roofs, whose rafters bear against the exterior of the brick cone, above the arches which connect the pillars. This extension constitutes the manufacturing workshop, or space occupied by the glass-making operatives. The interior space around the furnace and within the pillars, is that occupied by the founders, or the men whose duty it is to fill the pots with raw materials for the production of glass, to urge the fire, to examine from time to time the state of fusion, and in short, to make from sand, alkali and lime, by the aid of intense heat, the material which the glass-making operatives subsequently convert by manipulation into glass.

For very many consecutive hours during the process of founding the raw materials, a thermometer placed at the greatest possible distance from the furnace, but within the area occupied by the founders, and freely suspended from a rod projecting from the interior surface of one of the brick pillars (a distance in the present instance = 20 feet 5 inches), will indicate a temperature varying from 316° to 325° of Fahrenheit. The founders have cool recesses, into which they frequently retire during their work, but the average of temperature here mentioned, viz. from 316° to 325° , and frequently very much beyond 325° , they bear without experiencing any inconvenience whatsoever. Strangers universally wonder at the possibility of human beings existing in a situation in which their clothes are continually scorched, while their naked skin exhibits no marks of the effects of fire. I had myself often wondered at the circumstance, until I made some experiments to endeavour to ascertain the cause of such an anomaly. The results of some of these experiments are curious from the extent of the ranges of the temperatures, and I have much plea-

sure in proffering them to those of my philosophical brethren who may feel an interest in such matters.

Exp. 1. Two silvered brass scale thermometers, having each a range of 600° Fahrenheit, were suspended from an iron pin at a distance of two inches from the interior surface of one of the brick pillars of the glass-house, at a distance of 20 feet 5 inches from the nearest point of the furnace emitting flame, and during the early part of the founding process.

They both indicated, from an average of ten simultaneous pairs of consecutive observations which ranged well together, a temperature of $194^{\circ}\cdot4$ Fahrenheit.

Exp. 2. One of the thermometer bulbs was now clothed with a thin case of fine black "merino." The average of ten simultaneous pairs of consecutive observations indicated a difference of temperature manifested by the clothed bulb, in excess of that manifested by the unclothed bulb, of $23^{\circ}\cdot1$ Fahrenheit upon each pair of observations. Hence $23^{\circ}\cdot1$ of Fahrenheit were retained by the covered bulb, which were evidently reflected, and lost to observation, by the bright metallic surface of the unclothed bulb.

In another series of experiments wherein the temperature indicated by the unclothed thermometers averaged only $124^{\circ}\cdot5$ Fahrenheit, from twelve pairs of observations, the increment shown by covering one of the bulbs with a thin bag of black "merino," amounted to $34^{\circ}\cdot66$ Fahrenheit. Hence the quantity of heat that is reflected from the bright surface of a thermometer diminishes as the heat itself increases.

Exp. 3. During the latter part of the founding process and whilst the clothed thermometer suspended from the brick pillar ranged from 320° to 325° Fahrenheit, a small black iron cylindrical pan filled with water was placed upon a thin iron shelf, which had been fixed against the pillar and close by the side of the thermometer. It was reasonably anticipated that water thus placed in a temperature of 320° to 325° would boil; but after waiting until the half of it had evaporated, it showed no tendency to ebullition.

Exp. 4. The top of the iron pan was now covered with a pane of window-glass, and in a few minutes it boiled violently. This experiment demonstrated that the cooling properties of rapid evaporation can neutralize one of the direct effects of heat to a very surprising extent; and to ascertain the amount of this influence the following experiment was instituted.

Exp. 5. A clothed thermometer, whilst the mercury was indicating a temperature of 310° Fahrenheit, was immersed

in a vessel of boiling water. The mercury *instantly* fell to 212° , and then *very gradually* sunk to 141° . The merino envelope had become dry, and the mercury had commenced rising when the bulb was immersed a second time into the water. The mercury rose to 202° , and then gradually fell to 139° . By a third immersion the mercury rose to 198° , whence it fell *gradatim* to 133° . The envelope was now saturated with water at about 140° Fahrenheit, but the mercury speedily reassumed the temperature of 133° Fahrenheit, and remained at this fixed point for nearly five minutes, although the real temperature of its situation was, and had been for many previous hours, 310° Fahrenheit.

The effects of rapid aqueous evaporation were thus clearly shown to influence the indications of the thermometer when placed in a dry atmosphere of 310° Fahrenheit, and under the circumstances described, to the surprising extent of 177° Fahrenheit.

We may now infer that the *copious perspiration* which exudes from the skin of glass-makers, and of those who are engaged in similar scorching occupations, is a sufficient protection from the burning effects of a *dry* atmosphere of from 300 to 400 degrees of Fahrenheit; and that whilst the clothes of such persons are burning to tinder, their skin may be rendered insensible to the direct effects of fire upon the inanimate matter around them, by simple natural laws, viz. those of evaporation.

Having been engaged in some delicate experiments on the subject of heat, I was surprised at the effects of comparatively moderate *dry* temperatures upon such thermometer scales as were made of ivory.

In one instance the scale became shortened two degrees in 100 in a temperature of 212° Fahrenheit.

In another, an old and "well-seasoned" ivory scale that had often endured the maximum solar heat of Jamaica and the salt waters of the Atlantic, became shortened one degree in 110° from a *dry* temperature of 130° Fahrenheit.

In a third, the scale became shortened $1\frac{1}{2}^{\circ}$ in 120° from a short exposure to a *dry* heat of 260° Fahrenheit. Immersion in water will generally restore such scales to their original length.

I have the honour to be, Gentlemen,
yours obliged,

Wraxall, near Bristol,
April 22, 1840.

CHARLES THORNTON COATHUPE.

XXI. *Remarks on some Tide Observations, published in the Transactions of the British Association.* By RICHARD THOMAS, *Civil Engineer*.*.

IN the Transactions of the British Association for the year 1838, is an account of a level line measured from the Bristol Channel to the English Channel, from which the writer draws the conclusion, "that *the mean tide must be taken as the level of the sea,*" and that this mean tide is the same, or nearly the same elevation at all places, whatever may be the differences of rise and fall.

About three years ago the British Association published the results of certain tide observations made at Liverpool, which induced me to write the following letter on the subject, which was inserted in the 'Mining Journal,' and "Falmouth Packet" Newspapers.

"In the supplement to the 'Mining Journal,' No. 23, there appears to be published under the sanction of the British Association, '*that there is one invariable mean height common to neap and spring tides, the HALF TIDE MARK,* a point from which engineers, geologists, and navigators will henceforward commence their calculations and adjust their standards of comparison.'

"I have reason to believe that however accurate the conclusion is with regard to the tides at Liverpool, where the observations were made, it is not correct as to its general application, and I mean to show that the tides generally have not the same elevation of half-tide mark, as applies to any particular locality, nor is the average half-tide mark, nor low-water mark, nor high-water mark at one part of the coast to be depended on as level with the corresponding tide marks on other parts. More than twenty years ago I had occasion to attend particularly to the tides at Falmouth, and the result of my observations showed differences as much as two feet and a half in elevation on half-tide marks. The rise and fall of ordinary spring tides there is about 17 feet, and of ordinary neaps little more than seven feet, *but the several rounds of tides differ as to the mean elevation of the sea;* the low- and high-water marks for the same difference of rise and fall being at greater elevations than others.

"At King-Road near Bristol, I observed the tides in the year 1815, and found that the difference of elevations of some half-tide marks amounted to about four feet.

"It may be possible, and I think it probable, from my observations, that these differences of half-tide levels, or rather

* Communicated by the Author.

of the mean height of the sea, may be operated on in a regular way by whatever causes it to vary at different periods : but I am not aware that any observations have yet been made for the purpose of determining the recurrence of such variations, or for ascertaining their causes; and until this be done, no mean tide-level could be determined on. But, after all, such standard level must have reference to some fixed or *bench* mark in order to make it useful, and could only be applied to one neighbourhood, as it could not be depended on with reference to tides at any distant part of the coast, which may be exemplified by a reference to the tides between the Land's-end and King-Road; and although this example may be taken as almost an extreme case, nevertheless it will serve the better to elucidate the uncertainty of tide-marks.

“ It must be evident that the particular formation and trending of the coast as connected with the set of the tide will tend to alter the elevation of the high-water as well as of the low-water marks. The Bristol Channel, from its form narrowing upwards to the Severn, causes the tidal wave to rise much higher than at any other part of the English coast, for it may be considered that the tides at the flood have a tendency to rush from the Atlantic into the mouth of this channel with about the same velocity as through the inlets to the English Channel and the Irish Sea; but in the Bristol Channel there is not the same proportion of space as compared with the breadth of the entrance; hence in their course upward, being confined by the approximation of the shores, they are raised to a height increasing as the breadth lessens, until they attain sufficient elevation for the gravity of the water to counterbalance the impulse of the tide.

“ The sea at ordinary spring tides rises and falls about 45 feet at King-Road, yet the low water there appears to be some feet above the low water of the ocean, as the great influx of the Severn at such tides occasions a great quantity of water to be coming down even at the return of the flood, so that at the time of low water below the Holmes Isles, the down tides run with such rapidity at King-Road as to indicate a great fall, and consequently there is a considerable rise in the tide before it meets the level of the water there; but it must be recollected, that then there will be some fall in the water from the Holmes at King-Road.

“ At Ilfracombe, about the same time as I observed the tides at King-Road, the rises and falls were taken by my particular directions, and showed the ordinary spring tides there to rise and fall about 30 feet, and the ordinary neaps about 14 feet.

“ For reasons before stated, it appears that the level of the low water at King-Road is higher than the low water further down the channel. Now the half-tide mark of ordinary spring tides at King-Road is about 22 feet above the low water there. At Ilfracombe the half-tide is 15 feet above the low water, and at the Land's-end it is eight feet and a half.

“ Even supposing the low-water mark to be as low at King-Road as at the other places, then the half-tide there is seven feet above that mark at Ilfracombe, and thirteen feet and a half above that at Land's-end; but in all probability instead of 7 and $13\frac{1}{2}$, the differences are really about twelve and twenty.

“ The low-water marks of the corresponding tides appear to present a much better approximation to a true level than the half-tide mark, and these are taken for determining the elevations ascertained in the Trigonometrical Survey of England and Wales: but it would be desirable for many purposes that the movements of the tides should be better known, which might lead us to a knowledge of a true level, or so near an approximation as would answer all practical purposes.”

“ Dated November 14, 1837.”

Referring to the Transactions of 1838, as before mentioned, and granting that the series of levels have been ascertained with sufficient exactness for the purpose, we must draw the conclusion, either that the low water at King-Road at spring tide is about 13 feet below the low water at Land's-end, or that there is something which affects the tides at Axmouth so as to make the mean tide there stand at a higher elevation than it does at the Land's-end, and so make it approximate to the level of the mean tide at King-Road.

It appears to me very probable that the level of the sea is kept up at Axmouth above its average height, owing to the contour of the coast of the English Channel; for it is evident that the coast of Normandy, extending from Granville northward to Cap la Hogue, presents a bar to the direct flowing up channel of the flood tide, and turns the current towards that part of the English coast on which Axmouth is placed, and that the free course downward of the ebb-tide is interrupted by the projection southward of the coast of Devon between Exmouth and the Start; both of which causes will tend to keep the water the more elevated in the bay between the Start and the Bill of Portland. At the time of high water at Axmouth the tide is setting strongly up channel, and continues running up for, I believe, more than two hours longer, and the ebb runs down strongly at the time of low water at Axmouth. Now if the currents operate in the way above stated, it is

clear that the effect will be more apparent on spring tides than on neaps, owing to the greater velocity of the water, and so it appears to be.

On the south coast of Cornwall the rise of ordinary spring tides is about 16 to 17 feet, and of ordinary neaps a little more than 7 feet. At Axmouth the rise of spring tides appears to be about 10 or 11 feet, and of neaps about 7 feet. Hence we see that although the rise and fall of neaps at Axmouth nearly corresponds with that of the south coast of Cornwall, yet there is a difference of six feet in the comparison of spring tides; this appears strongly corroborative of the idea of the tide currents operating in the way I mention.

By a reference to the table of contemporaneous observations of the tides (plate 2.) accompanying the paper referred to in the Transactions of the British Association, it will be seen that the high- and low-water marks at Plymouth are so placed that the line of mean tide shall about coincide with that of the tides at Axmouth. There appears to be no authority for this, as it does not appear that any levels have been extended to Plymouth. For the reasons as above stated, I should conclude that the high-water mark at Axmouth would probably be higher than the high water at Plymouth; but supposing it the same, then in the series of tides observed, would the mean level at Plymouth be at neaps nearly one foot, and at springs nearly two feet lower than at Axmouth; and be it remembered that the greatest rises at Plymouth, as stated in this table, are only 14 feet, which I should consider as much less than the average rise of spring tides there, seeing that on the neighbouring coast of Cornwall it is nearly three feet more. I should also think that the high water at Axmouth is more elevated than at Falmouth; but if it be only at the same level, then will the mean-tide mark be three feet higher at Axmouth than at Falmouth.

In the tide-table above referred to, it appears that the mean-tide line at King-Road is about one foot above that at Axmouth. In this comparison the greatest tides observed at King-Road rise about 36 feet, in which series the greatest difference of mean-tide level does not amount to one foot. Other tides appear by the table to have been observed there, some of which had a rise of 41 feet, and the range of mean tide differed as much as two feet and a half. In the year 1815, as before stated, I observed a series of tides at King-Road, which ranged from a rise of 19 feet to a rise of 46 feet, differing in their respective elevations of mean tide as much as $3\frac{1}{2}$ feet; the greatest tide rising from 0 on the scale to 46

feet, and the smallest from 10 to 29 feet; the differences in the levels of low water being 10 feet, and of high water 17 feet.

Had the differences between the levels of low water of spring tides and of neap tides at King-Road and at the Land's-end been about the same, we might be led to conclude that all the difference of rise and fall would have been due to the increased elevation of high water, owing to the narrowing of the Bristol Channel in proceeding upwards, as before stated; but as the differences of levels of low water at King-Road amount to about ten feet and those at Land's-end only to about five feet, it may be possible that the momentum of the ebb-tide may draw the water down at King-Road at low water to below the level of low water at Land's-end. If it be so then there can be no sure means of comparisons of sea levels at either mean tide, high water, or low water; but I should consider that the low water of neap tides is most likely to be the nearest approximation to a true level, and that the mean level of the sea on the south-west coast of Great Britain is about three feet and a half above the average level of the low water of neap tides.

In order to ascertain the particular operations of the tides, and their effects in certain localities, it would be desirable to have the tide levels simultaneously taken at convenient places on both the north and south coast, extending from King-Road and from Axmouth to the Land's-end, marking the time and level of high and low water, and the elevation of the sea at every quarter or half hour throughout the day. These observations being connected at each place with some particular bench-mark, would furnish data for comparison whenever lines of levels might be extended to them. Such lines of levels might be surveyed across Cornwall in sundry places, and might ultimately be connected with the line already surveyed from Portishead to Axmouth.

Although I do not mean to impugn the correctness of the results of the leveling across the country, yet I think another line of levels should have been taken by way of verification before any conclusion should have been adopted as to comparison of tides. Such verification would prove the correctness of the relative levels of the several bench-marks, which might be applied to further observations on the tides, and the levels extended to other places. It appears that the operator took every care to observe correctly the levels, and to prove them by leveling back over the line, in which the difference of results are so trifling as not to excite notice, if it had not

been for the curious anomaly of the heights of the stations constantly coming out less on returning to them than on leaving them, which the operator suspects might have happened from the alterations of refraction. But surely he could easily have detected errors arising from such source by looking again at the back staff after he had observed the elevation on the fore staff.

To say nothing of the absurdity of supposing that the refraction was always decreasing during the survey, which it must have been to have brought about such results, the distances from the instrument to the staff (44 fathoms) were so small, that even supposing every day during the hours of work the refraction should at first have amounted to an angle equal to half the contained arc of the earth's surface, and should have decreased to nothing by the time the day's work was ended, and allowing a quarter of an hour to elapse between taking the back and fore observations, the error arising from refraction would not have amounted to half an inch on the whole distance from Portishead to Axmouth; nor would the daily shrinkage in the length of the staffs account for it, as such difference could only affect the levels in a very much less degree than the difference by the survey. It is possible that there might be some very small defect in the movements or the mounting of the instrument, so as in turning it round from the back to the fore observation it might gain in elevation; but the error appears to be so very small that it cannot materially affect the results.

Falmouth, April 30, 1840.

XXII. *Observations on the Blood Corpuscles, or Red Particles, of the Mammiferous Animals.* By GEORGE GULLIVER, F.R.S., F.Z.S., Assistant Surgeon to the Royal Regiment of Horse Guards. No. IV.

[Continued from page 200 of the Phil. Mag. for March 1840.]

137. **A** NEW species of Gibbon, (*Hylobates* — ?) a female. The most common diameters of the corpuscles 1-3600th, 1-3555th, 1-3429th, and 1-3200th of an inch. Extreme sizes 1-5333rd, and 1-2900th. Blood from a prick of the finger.

138. White-nosed Monkey, (*Cercopithecus petaurista*), a male, apparently an adult. The most frequent diameters of the disks 1-3555th, 1-3428th, and 1-3200th of an inch. Extreme sizes 1-4570th and 1-3000th. Blood from a prick of the tail.

139. Marikina or Silky Tamarin, (*Midas Rosalia*), an adult

female. Common-sized disks 1-3693rd, 1-3429th, and 1-3332nd of an inch. Extreme diameters 1-5333rd and 1-2666th. Blood from a prick of the tail.

140. Ring-tailed Lemur, (*Lemur catta*), an adult male. All the following sizes common, 1-4000th, 1-3840th, 1-3644th, 1-3600th, 1-3557th, 1-3429th of an inch. Extreme diameters 1-6000th and 1-3000th. There were several corpuscles about 1-8000th of an inch in diameter, but it is doubtful whether these were true blood disks.

The size of the corpuscles was altogether remarkably irregular. The blood was obtained from a prick of the ear: the animal was apparently not very healthy.

In some blood obtained from another full-grown male, a few hours after death, the corpuscles were also very irregular in size, but the most abundant disks were chiefly of small and large dimensions, the former being generally 1-5000th of an inch in diameter, and the latter 1-3500th. There were also a few about 1-7000th of an inch, and also some as large as 1-3000th. The majority, however, were the small and large kinds first noted, with but very few intermediate gradations. The smaller corpuscles presented a remarkably dark and distinct outline. Blood from the heart, from the coronary veins, as well as from the renal and mesenteric veins.

141. Black-fronted Lemur, (*Lemur nigrifrons*), a full-grown male. The following diameters all common: 1-4000th, 1-4500th, 1-4662nd, and 1-4705th of an inch. Extreme sizes 1-6000th and 1-3500th.

Blood from the tail in the first trial; and in the second, made a month afterwards, from a vein of the ear.

142. Anjouan Lemur, (*Lemur Anjuanensis*), a male, nearly full-grown. Common sizes 1-4365th, 1-4268th, 1-4000th, and 1-3555th of an inch. Extreme diameters 1-5333rd and 1-3200th.

Blood from a wound of the tail; also from a prick of the hand.

In the Lemurs the corpuscles were remarkable as being very variable in size. Whether this will be confirmed by future observation, remains to be seen. It is deserving of notice that most of the animals were more or less diseased, and died very rapidly, chiefly of tubercular consumption, in connection with which the blood particles were perhaps more or less modified.

In the black-fronted Lemur (141.) the disks appeared, from the result of two trials made at different times, to be smaller than in the other Lemurs.

In another examination of some blood obtained from the heart of an adult white-fronted Lemur (60.), the corpuscles

were again observed to be very variable in magnitude, 1-4800th, 1-4000th, 1-3600th, and 1-3000th of an inch, being all equally common diameters of the disks.

143. Slender Loris, (*Loris gracilis*), a male, apparently full-grown. The common-sized corpuscles 1-3555th, and 1-3200th of an inch in diameter. Extreme sizes 1-4600th and 1-2900th. Blood from a prick of the finger.

144. Common Ferret, (*Mustela furo*), an adult female. Common diameters 1-4800th, 1-4500th, 1-4000th, and 1-3693rd of an inch. Extreme sizes 1-6000th and 1-3000th.

Blood from a prick of the nose. The size of the disks was extremely variable.

145. Cape Hunting Dog, (*Lycaon tricolor*), a young male, probably about half-grown. The majority of the corpuscles 1-4000th and 1-3693rd of an inch in diameter. Extreme sizes 1-4570th and 1-3200th. Blood from a prick of the nose.

146. Spotted Hyæna, (*Hyæna crocuta*), an old male. The most frequent diameters 1-4360th, 1-4000th, 1-3600th, and 1-3555th of an inch. Extreme sizes 1-5333rd and 1-2900th.

Blood from a prick of the upper lip.

147. Virginian Opossum, (*Didelphis Virginiana*), a male, about two-thirds grown. Common diameters 1-3600th and 1-3530th of an inch. Extreme sizes 1-4570th and 1-2900th.

There were quantities here and there of the red granular particles, generally about 1-5000th of an inch in diameter, being as usual smaller than the regular disks.

The blood was obtained from a prick of the tail. The animal was diseased, the hind legs being paralysed.

148. Vulpine Opossum or Phalanger, (*Phalangista vulpina*), an adult male. Majority of the disks 1-4000th and 1-3693rd of an inch in diameter. Extreme sizes 1-5333rd and 1-3000th.

Blood from a prick of the ear.

149. Black Rat, (*Mus Rattus*), an adult male. The most frequent diameters 1-4000th, 1-3692nd, and 1-3448th of an inch. Extreme sizes 1-5000th and 1-3000th. Blood from an incision of the tail.

150. Splendid Flying Squirrel, (*Pteromys nitidus*), a full-grown female. The common sized disks 1-4000th, 1-3600th, and 1-3555th of an inch. Extreme diameters 1-4570th and 1-3200th. The size of the disks was remarkably regular. Blood from a prick of the tail.

151. Spotted Cavy or Paca, (*Cælogenys subniger*), a male, apparently an adult. The most frequent diameters 1-3693rd, 1-3600th, 1-3429th, and 1-3330th of an inch. Extreme sizes 1-4000th and 1-3000th. Blood from a cut of the tail.

152. Roebuck, (*Cervus capreolus*), an adult male. All the

following diameters common: 1-6000th, 1-5800th, 1-5332d, 1-5000th, 1-4924th, and 1-4800th. Extreme sizes 1-6400th and 1-4000th of an inch.

Blood from a vein of the ear, also from a prick of the velvet of the young growing antler. The corpuscles obtained from these different parts were identical in size and appearance.

A recent examination of the blood corpuscles of some varieties of the common sheep (42.) gave the following results.

a. A four-horned Sheep from North Africa.

b. A two-horned hairy variety from Africa, presented to the Zoological Society by the Duke of Sutherland.

c. A hairy variety from Demerara.

All the above were full-grown males, and the blood was obtained from small incisions of the lips. The corpuscles in the different varieties appeared to be of the same size; the most frequent diameters being 1-6000th, 1-5615th, 1-5331st, and 1-5028th. Extreme sizes 1-7110th and 1-4570th of an inch.

XXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 27, **T**HE reading of a paper entitled, "On the Chemical Action of the Rays of the Solar Spectrum on Preparations of Silver and other Substances, both metallic and non-metallic; and on some Photographic processes;" by Sir John F. W. Herschel, Bart., V.P.R.S., &c., was resumed but not concluded.

March 5.—The reading of a paper entitled, "On the Chemical Action of the Rays of the Solar Spectrum on Preparations of Silver and other Substances, both metallic and non-metallic; and on some Photographic processes;" by Sir John F. W. Herschel, Bart., V.P.R.S., &c., was resumed and concluded. An abstract of this paper has already appeared in vol. xvi. p. 331 of this Magazine.

A paper was also read entitled, "Remarks on the Theory of the Dispersion of Light, as connected with Polarization;" by the Rev. Baden Powell, M.A., F.R.S., and Savilian Professor of Geometry, Oxford.

Since the publication of a former paper on the subject referred to, the author has been led to review the subject in connexion with the valuable illustrations given by Mr. Lubbock of the views of Fresnel; and points out, in the present supplement, in what manner the conclusions in that paper will be affected by these considerations.

A paper was also read, entitled, "Further Particulars of the Fall of the Cold Bokkeveld Meteorite;" by Thomas Maclear, Esq., F.R.S., in a letter to Sir John F. W. Herschel, Bart., K.H., V.P.R.S., &c. Communicated by Sir John Herschel.

This communication, which is supplementary to the one already

made to the Society by Mr. Maclear*, contains reports, supported by affidavits, of the circumstances attending the fall of a meteoric mass in a valley near the Cape of Good Hope. The attention of the witnesses had been excited by a loud explosion which took place in the air, previous to the descent of the aerolite, and which was attended by a blue stream of smoke, extending from north to west. Some of the fragments which had been seen to fall, and which had penetrated into the earth, were picked up by the witnesses. One of them falling on grass caused it to smoke; and was too hot to admit of being touched. The mass which was sent to England by H.M.S. Scout, weighed, when first picked up, four pounds. The paper is accompanied by a map of the district, showing the course of the aerolite.

A paper was also read, entitled, "An account of the Shooting Stars of 1095 and 1243;" by Sir Francis Palgrave, K.H., F.R.S., &c.

The author gives citations from several chronicles of the middle ages, descriptive of the remarkable appearance of shooting stars which occurred on the 4th of April, 1095, on the testimony of independent witnesses both in France and England. One of them describes them as "falling like a shower of rain from heaven upon the earth:" and in another case, a bystander, having noted the spot where the aerolite fell, "cast water upon it, which was raised in steam, with a great noise of boiling." The Chronicle of Rheims describes the appearance as if all the stars in heaven were driven, like dust, before the wind. A distinct account of the shooting stars of July 26th, 1293, is given by Matthew Paris.

March 12.—A paper was read, entitled, "On certain variations of the mean height of the Barometer, mean temperature and depth of Rain, connected with the Lunar Phases, in the cycle of years from 1815 to 1823." By Luke Howard, Esq., F.R.S.

The table given in this paper contains the results of calculations relating to the objects specified in the title; cast into periods of six, seven, or eight days, so as to bring the day of the lunar phase belonging to it in the middle of the time. The observations were all made in the neighbourhood of London. It appears from them that in the period of the last quarter of the moon the barometer is highest, the temperature a little above the mean, and the depth of rain the smallest. In the period of the new moon, both the barometer and temperature are considerably depressed, and the rain increased in quantity. The influence of the first quarter shows itself by the further depression of the barometer; but the temperature rises almost to the point from which it had fallen, and the rain still increases, but not in an equal ratio. Lastly, the full moon again reduces the temperature; while the barometer attains its maximum mean height, and the quantity of rain is the greatest. Thus it appears, that during this lunar cycle, the approach of the last quarter is the signal for the clearing up of the air, and the return of sunshine.

[* See L. and E. Phil. Mag., vol. xiv. p. 368, for an abstract of Mr. Maclear's former paper.]

A paper was also read, entitled, "On the theory of the dark bands formed in the solar spectrum from partial interception by transparent plates." By the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford.

This paper contains the mathematical investigation of the phenomena of peculiar dark bands crossing the prismatic spectrum, when half the pupil of the eye, looking through the prism, is covered by a thin plate of any transparent substance, the edge being turned from the violet towards the red end of the spectrum; and which were first noticed by Mr. Fox Talbot, and were ascribed by Sir David Brewster to a new property of light, consisting of a peculiar kind of polarity.

The author shows, that on the undulatory theory, in all cases, a difference of retardation between the two halves of each primary pencil throughout the spectrum may give bands within certain limits; and that it affords a complete explanation of the phenomena in question.

March 19.—A paper was read, entitled, "Contributions to Terrestrial Magnetism." By Major Edward Sabine, R.A., V.P.R.S.

An increased activity has recently been given to researches in terrestrial magnetism, with the definite object of obtaining correct maps of the magnetic phenomena, corresponding to the present epoch, over the whole surface of the globe. To aid these researches, and to facilitate the comparison of the general theory of M. Gauss with the facts of observation, maps have been constructed of the magnetical lines, both as computed by the theory, and as derived from observations already obtained. The theoretical and actual lines of the declination and intensity have thus been represented in maps recently published in Germany and England, as have also the lines of the inclination computed by theory; but the corresponding map or the latter element derived from observations is yet wanting. The object of the present communication is to supply this desideratum, as far as regards the portion of the globe contained between the parallels of 55° N. and 55° S., and the meridians of 20° E. and 80° W.; comprising the Atlantic ocean, and the adjacent coasts of the continents on either side.

The observations chiefly employed for this purpose are two series *made at sea*; one by Mr. Dunlop of the Paramatta observatory, in a voyage from England to New South Wales, in 1831; the other by Lieut. Sullivan of the Royal Navy, in a voyage from England to the Falkland Islands, and back, in 1838 and 1839. The observation of the magnetic dip at sea, which was commonly practised by the distinguished navigators of the last century, was unfortunately not resumed when the interest in such researches was revived on the restoration of peace: but it is by such observations only that the lines of inclination can be independently traced over those large portions of the globe which are covered by the ocean. The difficulties which attend the observation, occasioned by the motion and the iron of a ship, require the adoption of several precautions, which it is particularly desirable at this time to make generally known. The series of

Messrs. Dunlop and Sullivan are discussed in this view; and the value of results obtained under circumstances of due precaution is pointed out by their success.

The position of the lines on the land portion of the map is derived from 120 determinations in various parts of Europe, Africa, and America, between the years 1834 and 1839, of which about the half are now first communicated.

The series of Messrs. Dunlop and Sullivan contain also observations of the magnetic intensity made at sea; Mr. Dunlop's by the method of horizontal vibrations, and Lieut. Sullivan's by the instrument and method devised by Mr. Fox. The degree of precision which may be obtained by experiments thus conducted, is shown by the comparison of these observations with each other, and with the isodynamic lines previously derived from observations made on land.

The first section of this paper concludes with discussions on the relative positions of the lines of least intensity and of no dip, and of the secular change which the latter line has undergone in the ten years preceding 1837.

In the second section, the observations of Mr. Dunlop are combined with recent observations on the coasts of Australia, by Captains Fitz Roy, Bethune, and Wickham, of the Royal Navy, to furnish a first approximation to the position and direction of the isodynamic lines over that portion of the Indian ocean which is comprised between the meridian of the Cape of Good Hope and New South Wales.

A paper was also in part read, entitled, "Experimental Researches in Electricity, seventeenth series. By Michael Faraday, Esq. D.C.L., F.R.S., &c. On the source of power in the Voltaic Pile."

March 26.—The reading of a paper, entitled, "Researches in Electricity, Seventeenth Series: on the source of power in the Voltaic Pile." By Michael Faraday, Esq., D.C.L., F.R.S., &c., was resumed and concluded.

In a postscript, the author states that he has since found a passage in Dr. Roget's treatise on Galvanism, in the Library of Useful Knowledge, published in January 1829, in which the same argument respecting the unphilosophical nature of the contact-theory is strongly urged.

"Were any further reasoning necessary to overthrow it, (namely, the voltaic theory of contact) a forcible argument might be drawn from the following consideration. If there could exist a power, having the property ascribed to it by the hypothesis, namely, that of giving continual impulse to a fluid in one constant direction, without being exhausted by its own action, it would differ essentially from all the other known powers in nature. All the powers and sources of motion with the operation of which we are acquainted, when producing their peculiar effects, are expended in the same proportion as those effects are produced; and hence arises the impossibility of obtaining by their agency a perpetual effect, or, in

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other words, a perpetual motion. But the electro-motive force ascribed by Volta to the metals when in contact, is a force which, as long as a free course is allowed to the electricity it sets in motion, is never expended, and continues to be exerted with undiminished power, in the production of a never-ceasing effect. Against the truth of such a supposition the probabilities are all but infinite.”—§ 113, p. 32.

April 2.—The following papers were read, viz. :

“Additional note to the Eleventh Series of Researches on the Tides.” By the Rev. William Whewell, B.D., F.R.S., &c.

As an appendix to his former memoir on tide observations*, the author gives in the present paper the results of observations made at Petropaulofsk, in the bay of Avatcha, in Kamtchatka, lat. $53^{\circ} 1' N.$, long. $158^{\circ} 44' E.$, by the officers and men of the *Seuivine*, commanded by the present Russian Admiral Lütke; and which were conducted with great care and perseverance. The height of the surface was noted every ten minutes, both day and night, and when near its maximum every two minutes.

It appears from these observations that the high water is affected in its time by a very large diurnal inequality, reaching the enormous amount of above four hours; while its height is only slightly affected by an inequality of that kind; the greatest alternate inequalities of height were something more than a foot. In the low waters, there appears a much smaller inequality in the times, seldom amounting to more than one hour; but with regard to height, the diurnal inequality is much larger than that for high water, reaching to three, or even four feet; and this in a tide of which the whole rise, from the lowest to the highest, rarely exceeds five feet. The theory of these phenomena is then discussed.

The results of another series of observations made in July 1827, at the port of Novo-Arkhangelsk, in the island of Sitkhoe, in Norfolk sound (lat. $57^{\circ} 2' N.$, long. $135^{\circ} 18' W.$), are also given, and their theory considered.

A paper was also in part read, entitled, “On the Nervous System.” By Sir Charles Bell, F.R.S.

April 9.—The reading of a paper, entitled, “On the Nervous System.” By Sir Charles Bell, F.R.S., was resumed and concluded.

The author adverting to the papers on the nervous system, which he presented to the Royal Society nearly twenty years ago†, recapitulates the train of reasoning which originally led him to the inquiries in which he has been so long engaged, on the different functions of different classes of nerves, and adduces various pathological facts in corroboration of the correctness of the views he then entertained. With regard to the spinal nerves, cases are related

* [Abstracts of Mr. Whewell's former Researches on the Tides will be found in the Lond. and Edinb. Phil. Mag., vol. xv. p. 316.]

† [See Phil. Mag., first series, vol. lxiv. pp. 41. 119. 353. 442; and Lond. and Edinb. Phil. Mag., and Annals, 2d series, vol. vi. p. 135; also Lond. and Edinb. Phil. Mag., vol. vii. p. 138.]

where, in consequence of disease of the bodies of the vertebræ, the anterior columns of the spinal chord, and anterior roots of the nerves were affected, and paralysis of the muscles to which those nerves are distributed was produced, while the posterior column of the cord was uninjured, and the sensibility unimpaired. The author next considers the respiratory system of nerves, which he regards as being both muscular and sensitive, and describes as arising from a tract of the spinal cord, on the outside of the *corpus olivare*, and anterior to the *processus ad cerebellum*; and which constitute columns having no decussations with one another, as is the case with the other systems. The conclusion he originally formed, that both the phrenic and the spinal accessory nerves are provided for motion, which he had deduced from the anatomical fact of the former taking a direct course to the diaphragm, and the latter a circuitous one for the purpose of associating the muscles of the respiratory organs with those which act on the chest, is, he thinks, amply confirmed by subsequent experiments. He concludes his paper with some remarks on the supply of blood to the respiratory system of nerves, which supply, being derived from branches of the vertebral arteries, affords an explanation of several pathological phenomena.

A paper was also read, entitled, "On the constitution of the Resins. Part IV."* By James F. W. Johnston, Esq., M.A., F.R.S.

This paper contains the account of the continuation of the author's previous researches into the constitution of the resins, both as they occur in nature, and as they appear when extracted from the natural products by the agency of alcohol or æther. The great difficulty in this inquiry is to determine when the resin to be analysed is obtained in its normal state; and the author has endeavoured in each case to ascertain this point by repeated analyses of the resins prepared under different conditions. He thus arrives at the conclusion, that the resin of scammony extracted from crude scammony by alcohol, and heated to 260° Fahr., is represented by $C_{40}H_{33}O_{20}$, containing the largest amount of oxygen of any resin hitherto analysed. The resin of jalap, obtained by evaporating the alcoholic extract, and afterwards boiling it in water, is represented by $C_{40}H_{34}O_{18}$, and in the amount of oxygen it contains is only surpassed by the resin of scammony. It is interesting to remark that these two resins in their effects on the animal œconomy are as nearly related as these formulæ show them to be in chemical constitution.

The resin of labdanum, extracted by alcohol from the crude labdanum and evaporated, gave the formula $C_{40}H_{33}O_8$; but this extract, softened in the air and water, took up from it a bitter substance of a brown colour. After boiling in water, the pure resin is represented by $C_{40}H_{33}O_7$.

The Berengela resin, previously analysed by the author before he was aware of the conditions necessary to be attended to in order to

* [See Lond. and Edinb. Phil. Mag., vol. xv. p. 327, for the former papers.]

obtain a resin in its normal state, is expressed by the formula $C_{40} H_{30} O_7$; and the resin of Retin asphalt, also previously analysed by the author, by $C_{40} H_{27} O_6$.

The resin of ammonia, extracted by alcohol from the crude gum resin, is represented by $C_{40} H_{25} O_9$; the resin of opoponax by $C_{40} H_{25} O_{14}$; and that of assafœtida by $C_{40} H_{26} O_{10}$.

A striking relation appears between the formulæ for the resins of ammonia and assafœtida, the former being $C_{40} H_{25} O_9$, the latter $C_{40} H_{26} O_{10}$, as if the latter were merely a hydrate of the former. The author considers this relation, and concludes that it is only apparent, and that probably in neither of the resins does any of the hydrogen exist in combination with oxygen in the state of water.

This leads the author to inquire into the general action of a slightly elevated temperature on the resins, and he concludes that in all cases when a resin in its normal state is heated a few degrees above its melting point, it begins to suffer partial decomposition, accompanied by the solution of water, and *always* by more or less of a volatile, generally oily compound, sometimes containing less and sometimes more oxygen than the resin which has been subjected to heat. In the case of some resins, especially such as are agreeably fragrant, and are expressed by the second of the author's general formulæ $C_{40} H_{24} \pm x O_7$, benzoic acid is one of the products of decomposition at a moderate temperature. Thus the resin of dragon's blood gives only a trace of benzoic acid, with water and a red volatile compound; while the resin of benzoin gives much benzoic acid. Some resins give off volatile matters and diminish in weight long before they reach the fusing point; as is the case with the resin of benzoin, of which the melting point is high. With regard to the special action of such temperatures in altering the atomic constitution of the resins, the author finds that each resin undergoes a change, probably peculiar to itself, and probably depending on the nature of the organic radical it contains. Thus, the formula for the resin of retin asphalt ($= C_{40} H_{27} O_6$) by prolonged heating at the melting point becomes $C_{40} H_{27} O_5$. Ammonia resin ($= C_{40} H_{25} O_9$) by heating at 270° Fahr. approaches to $C_{40} H_{24} O_8$; there being, however, a slight excess of oxygen, and water not being the only volatile compound driven off.

The resin of opoponax, when thus heated the hydrogen, as in that of retin asphalt, remains nearly constant $= C_{40} H_{25} O_{14}$, approaching to $C_{40} H_{25} O_{12}$. The same is the case with the resin of assafœtida ($= C_{40} H_{26} O_{10}$), which by prolonged heating at about 250° Fahr., becomes $C_{40} H_{26} O_9$. These observations when multiplied are likely to assist materially in leading to *rational* formulæ, expressive of the molecular constitution of the resins.

In reference to the general questions, with a view to the solution of which the author undertook this investigation, he concludes:

1. That the resins are not to be considered as different compounds of one and the same radical, but rather as analogous groups of compounds of analogous radicals.

2. That as far as our present knowledge extends, all the *true* resins are capable of being represented by irrational formulæ, in which C_{40} is a constant quantity.

3. That the analyses contained in the present paper render necessary a slight modification in the general formulæ previously announced. The formula for the group of which colophony is the type, being $C_{40} H_{32} \pm x Oy$; and that for the group of which gamboge or dragon's blood is the type, being $C_{40} H_{24} \pm x Oy$.

The author announces a further continuation of these researches, in which the constitution of other resins will be given, and the relations of the resins to certain chemical reagents will be explained and illustrated.

The Society then adjourned over the Easter Recess, to meet again on the 30th of April.

GEOLOGICAL SOCIETY.

[Continued from p. 74 of the present volume.]

Dec. 18, 1839.—A paper was first read, entitled "Description of the fossil remains of a mammal, a bird, and a serpent, from the London clay," by Richard Owen, Esq., F.R.S., F.G.S.

The author commences by observing, that only a few months had elapsed since the highest organic animal remains known to exist in the London clay were those of reptiles and fishes; and that the danger of founding conclusions in Palæontology from negative evidence was perhaps never more strikingly illustrated than by the fact, that the first scientifically determined relic of a warm-blooded animal from that formation proved to belong to the highest order of that class, if man be excepted; and that besides those quadrumanous remains, there have since been discovered in the London clay underlying the coralline crag, near Kyson, in Suffolk, teeth of cheiroptera, and of a species probably belonging to the marsupial order*.

Mr. Owen then proceeds to describe the fossils, the immediate objects of the communication.

1. The portion of the mammal was discovered by Mr. Richardson in the cliffs of Studd Hill, near Herne Bay, and belongs to a new and extinct genus of Pachydermata. It consists of a small mutilated cranium about the size of that of a hare, containing the molar teeth of the upper jaw nearly perfect, and the sockets of the canines. The molars are seven in number on each side, and resemble more nearly those of the *Chæropotamus* than of any other known genus of existing or extinct mammalia. They present three distinct modifications of the grinding surface, and increase in complexity from before backwards. The first and second spurious molars have simple sub-compressed crowns, surmounted by a single median conical cusp, with a small anterior and posterior tubercle at the outer side, and a ridge along the inner side of its base. They are separated by an

* Annals of Natural History, Nov. 1839.

interspace nearly equal to the antero-posterior diameter of the first molar. The second and remaining molars are in close juxtaposition. The third and fourth molars form the principal difference between the dentition of the present genus and that of the *Chæropotamus*, being larger and more complex in the grinding surface. They present a sudden increase in size and change of form. The plane of the crown is triangular, with the base outwards, and the posterior and inner side convex: it supports three principal cusps, two on the outer, and one on the inner side; there are also two smaller elevations with a depression on the summit of each, situated in the middle of the crown, and the whole is surrounded with a ridge which is developed into a small cusp at the anterior and external angle of the tooth. The three true molars closely correspond with those of the *Chæropotamus*. The sockets of the canines indicate that these teeth were relatively as large as in the peccari.

The bones of the head are separately described: the palatal processes of the maxillary bones are shown to be rugous, as in the peccari; the eye to have been full and large, as indicated by the size of the optic foramen and the capacity of the orbit, equalling an inch in vertical diameter: the general form of the skull is described as partaking of a character intermediate between that of the hog and the hyrax, though the large size of the eye must have given to the physiognomy of the living animal a resemblance to that of the *Rodentia*.

These indications, Mr. Owen says, scanty though they be, of the form of a species nearly allied to the *Chæropotamus*, are extremely interesting, on account of the absence of similar information regarding that genus. The resemblance of the molar division of the dental system in the new genus, for which the name of *Hyracotherium* is proposed, and the *Chæropotamus*, is sufficiently close to warrant the conclusion, that the canines and incisors if not similar would differ only in form and proportion; and that hence it may be ventured to solve analogically some of the doubts entertained by Cuvier respecting the dental characters of the *Chæropotamus*, and to affirm confidently that it had canines in the upper as well as the lower jaw. The incisor teeth with the *ossa intermaxillaria* are wanting in the specimen of the *Hyracotherium*, and have not been found in any fragment of the *Chæropotamus*.

2. The remains of birds described in the paper consist of a sternum, with other bones, and a sacrum, the former belonging to the collection of the late John Hunter, in the Royal College of Surgeons, and the latter to the cabinet of Mr. Bowerbank. Both the specimens were obtained from Sheppey. The Hunterian fossil includes the sternum nearly entire, the proximal ends of the coracoid bones, a dorsal vertebra, the distal end of the left femur, the proximal end of the corresponding tibia, and a few fragments of ribs. Mr. Owen first shows, in approximating to which of the three great groups of birds, terrestrial, aerial, or aquatic, the *Ornitholite* belonged, that from the length of the sternum and the remains of the primary in-

termuscular crest or keel, it could not have been a strictly terrestrial bird, though these characters do not prove that it was a bird of flight, as they occur in the Penguins or other Brachyptera, which have need of muscular forces to work their wings as paddles under water. In the present fossil, however, from the lateral extent and convexity of the sternal plate, the presence and course of the secondary intermuscular ridges, the commencement of the keel a little way behind the anterior margin of the sternum, Mr. Owen says there is no affinity with the brachypterous family. The coracoid bones or posterior clavicles, he also shows are less available in determining the habits of the Ornitholite, as they relate much more closely to the respiratory actions than to the movements of the wings, and are strongly developed even in the Apteryx. There remained consequently for comparison the ordinary birds of flight; and of these, the native species, which resemble the fossil in size, first claimed Mr. Owen's attention. Though the sternum is not complete, yet sufficient remains to have enabled him to set aside the Gallinaceous, and those Grallatorial and Passerine birds which have deeply incised sternums, and to restrict the field of comparison to such species as have the sternum either entire, or with shallow posterior emarginations. After a rigid comparison of the minor structural details and pursuing it from the sea gulls and other aquatic birds upwards through the Grallatorial and Passerine orders, omitting few British species, and no genus, he at length found the greatest number of correspondences in the skeleton of the accipitrine species. The resemblance, however, was not sufficiently close to admit of the fossil being referred to any native genus of Raptores: the breadth of the proximal end of the coracoid removes it from the owls (*Strigidae*), the shaft of the same bone is too slender for the Falconidae; and the femur and tibia are relatively weaker than in many of the British Hawks or Buzzards. It is with the Vultures that Mr. Owen has found the closest agreement; but he says the fossil indicates a smaller species than any known to exist in the present day, and is probably a distinct subgenus.

The professed ornithologist, Mr. Owen remarks, may receive with reasonable hesitation a determination of family affinities arrived at, in the absence of the usual characters deduced from the beak and feet; but in the course of a long series of close comparisons, he says, he has met with so many more characters, both appreciable and available in the present problem, than he anticipated, that he confidently expects, in the event of the mandibles, the bones of the feet, or the entire sternum of the bird in question being found, they will establish his present conclusion, that the Sheppey ornitholite is referrible to a member of the group of Accipitrine Scavengers, so abundant in the warmer latitudes of the present world.

The Ornitholite in Mr. Bowerbank's museum consists of ten sacral vertebræ anchylosed together, as is usual in birds with a continuous keel-like spinal ridge. Four of the vertebræ are analogous to the lumbar vertebræ in the mammalia, and they are succeeded by five others, in which, as in the Vultures, the inferior transverse pro-

cesses are not developed. This character, however, Mr. Owen says, is not peculiar to the Vulturidæ. Though the part of the fossil preserved is eminently characteristic of the class of birds, yet it is not calculated to throw light on the closer affinities of the species to which it belongs: nevertheless it supports rather than affects the determination of the Hunterian specimen. For the apparently extinct bird indicated by these fossils, the name of *Lithornis vulturinus* is provisionally proposed.

3. Mr. Owen commences his description of the remains of an extinct species of Serpent found at Sheppey, by pointing out the essential characters by which the vertebræ of an Ophidian Reptile are distinguished.

Vertebræ joined enarthrodially by a deep anterior transversely oblong cup and a corresponding prominent posterior ball, and further articulated by projecting posterior oblique processes, wedged like the carpenter's tenon into a mortice, excavated in the anterior oblique processes of the succeeding vertebra, supporting moreover on each side of the fore part of the body an oblong convexity for the moveable articulation of the rib, can belong, Mr. Owen observes, to no other than a reptile of the Ophidian order.

One of the specimens described in this portion of the memoir, consists of about 30 vertebræ possessing the above characters; also of a number of long slender ribs, having expanded concave vertebral extremities cemented irregularly together by a mass of indurated clay, and it forms part of the Hunterian collection of fossils; another specimen, consisting of 28 vertebræ, and some others of less magnitude, belong to Mr. Bowerbank's collection. All the specimens, Mr. Owen considers, are referrible to the same species, and they were all found at Sheppey.

The vertebræ in each specimen present the same conformation, and nearly the same size, being equal in this respect to those of a Boa Constrictor 10 feet long. They belong to the ordinary dorsal or costal series, and differ from those of the Boa and Python in their superior length as compared to their breadth and height. The ridge continued from the anterior to the posterior oblique processes on each side is less developed: the oblique processes themselves do not extend so far outwards; and the spinous process is narrower in its antero-posterior extent but longer. In the first two of these differences, the fossil agrees with the Linnæan Coluber and its subgenera, but differs from the Crotalus; and in the remaining points it differs from Crotalus, Coluber, Naja and Trionocephalus. The long and comparatively narrow spine, the outward prolongation of the upper angle of the posterior oblique processes, the uniform convexity of the costal protuberance, the uneven or finely wrinkled external surface of the superior arch of the vertebra, are characters which distinguish these Ophidian vertebræ from those of any other genus of the order with which Mr. Owen has been able to compare them. He therefore proposes to call the species provisionally *Palæophis To-liapicus*.

The ribs are hollow as in all land serpents,

From the agreement in the configuration of the under surface of the body of the vertebræ of the fossil with that in the vertebræ of the Boæ and Pythons more nearly than with the Colubri, and in none of the differences above noticed indicating any obstacle to the entrapping and destroying a living struggling prey, as well as from the length (11 feet) which it may be inferred the creature attained, Mr. Owen concludes it was not provided with poisonous fangs. Serpents of similar dimensions exist in the present day only in tropical regions, and their food consists principally of the warm-blooded animals. Mr. Owen therefore in conclusion states, that had no evidence been obtained of birds or mammals in the London clay, he would have felt persuaded that they must have coexisted with the *Palæophis Toliapicus*.

ROYAL IRISH ACADEMY.

Feb. 24.—A communication was read, entitled, "Justification of Mrs. Somerville's Experiments upon the magnetizing Power of the more refrangible solar Rays*." By George James Knox, Esq. and the Rev. Thomas Knox.

Professor Morichini of Rome was the first to observe that steel, when exposed to the violet rays of the solar spectrum, becomes magnetic. Similar experiments were tried by Mr. Christie in 1824; but the most accurate experiments upon this subject have been performed by Mrs. Somerville in 1825, who determined that not only violet, but indigo, blue and green, develop magnetism in the exposed end of a needle, while yellow, orange, and red produce no sensible effect. As many philosophers have failed in repeating these experiments, we were induced, in the course of the summer, to undertake the investigation of this subject, "which has so often disturbed science." Having procured several hundred needles, of different lengths and thicknesses, and having ascertained that they were perfectly free from magnetism, we enveloped them in white paper, leaving one of their extreme ends uncovered. Taking advantage of a favourable day for trying experiments upon the chemical ray, (known by the few seconds required to blacken chloride of silver,) we placed the needles at right angles to the magnetic meridian, and exposed them for three hours, from eleven to one, to the differently refrangible rays of the sun, under coloured glasses. Those beneath the red, orange, and yellow, showed no trace of magnetism, while those beneath the blue, green, and violet, exhibited, the two first feeble, but the last strong traces of magnetism.

To determine how far the oxidating power of the violet ray is concerned in the phenomena, we exposed to the different coloured lights needles whose extremities had been previously dipped in nitric acid, and found that they became magnetic (the exposed end having been made a north pole) in a much shorter time than the others,

* Phil. Trans. vol. cxvi. 1826. (Reprinted in Phil. Mag. First Series, vol. lxviii. p. 168.)

and that this effect was produced in a slight degree under the red (when exposed a sufficient length of time), strongly under white glass; and so strong under violet glass, that the effect took place even when the needles were placed in such a position along the magnetic meridian, as would tend to produce, by the earth's influence, a south pole in the exposed extremity.

Conceiving that the inactive state produced in iron (as observed by Schœnbein) when plunged into nitric acid, sp. gr. 1.36, or by being made the positive pole of a battery, or by any other means, might throw some light upon the nature of the electrical change produced, experiments were instituted to this effect, which showed that no trace of magnetism could be thereby produced.

March 16. (Stated meeting.)—The following Gentlemen were duly elected Officers and Council for the ensuing year:—

President: Sir William Rowan Hamilton, LL.D.—*Treasurer*: Thomas Herbert Orpen, M.D.—*Secretary to the Academy*: Rev. Joseph H. Singer, D.D.—*Secretary to the Council*: James MacCullagh, LL.D.—*Secretary of Foreign Correspondence*: Rev. Humphrey Lloyd, A.M.—*Librarian*: Rev. William Hamilton Drummond, D.D.—*Clerk and Assistant Librarian*: Edward Clibborn.

Committee of Science:—Rev. Franc Sadlier, D.D., Provost of Trinity College; Rev. Humphrey Lloyd, A.M.; James Apjohn, M.D.; James MacCullagh, LL.D.; Rev. William Digby Sadleir, A.M.; Robert Ball, Esq.; Robert Kane, M.D.

Committee of Polite Literature:—His Grace the Archbishop of Dublin; Rev. Joseph Henderson Singer, D.D.; Samuel Litton, M.D.; Rev. William Hamilton Drummond, D.D.; Rev. Charles Richard Elrington, D.D.; Rev. Charles William Wall, D.D.; Rev. Thomas H. Porter, D.D.

Committee of Antiquities:—Thomas Herbert Orpen, M.D.; George Petrie, Esq., R.H.A.; Rev. Cæsar Otway; Very Rev. the Dean of St. Patrick's; Rev. James Henthorn Todd, D.D.; Henry J. Monck Mason, LL.D.; Aquilla Smith, M.D.

The President then appointed the following Vice-Presidents: His Grace the Archbishop of Dublin; the Provost of Trinity College; the Rev. Humphrey Lloyd; the Very Rev. the Dean of St. Patrick's.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

At the meeting of this society on Monday, June 1, Mr. Hopkins made a communication respecting certain geological phænomena of elevation, and their probable connexion with the existence of volcanos. When a district is traversed by a number of faults of which the directions are nearly parallel, and the planes of which deviate (as is generally the case) from verticality, the mass contained between any two adjoining faults will form a perfect or truncated wedge, according as the line in which the planes of the faults, or those planes produced intersect, lies within or without the elevated mass. A double system of wedges will thus be formed, of which

one part will have their thicker portions upwards and the other downwards. The latter will manifestly be acted on respectively by elevating forces greater in proportion to their mass than the former, supposing the uplifting force to act uniformly over the lower surface of the mass; and consequently after the fissures are produced by the elevation and extension of the mass, the lower system of wedge will be forced upwards more than the upper one, and thus the two systems will be relatively displaced in such a manner as to bring thicker portions of them in contact than in their original positions. The two systems being thus *jammed* into each other, the uplifted mass will be supported as an arch, provided the *abutments* on which the extremities rest be sufficiently firm to support the pressure thus thrown upon them.

If we conceive a vertical line to be drawn downwards from any point in which the plane of a fault meets the earth's surface, the line will not generally coincide with the plane; and it is found that the relative displacement of the portions of the mass on opposite sides of the fault is usually such, that that portion in which the above vertical line lies appears most elevated.

Mr. Phillips has established this law by numerous observations, as a very general one. In considering the relative positions of the two systems of wedges above-described, after their displacement, it will easily be seen how simply this law is generally thus accounted for.

It thus appears, also, how great horizontal forces, and consequently great horizontal displacements may be produced, for which it would be difficult perhaps so easily to account in any other manner.

Mr. Hopkins explained the probable bearing of the above views on the theory of volcanos in the following manner. The temperature of the earth, in accordance with actual observations at comparatively small depths, may be such at the depth of twenty or thirty miles, as would under the pressure of the atmosphere fuse most of the substances composing the surface of the globe. No complete fusion, however, of the matter of the earth at the above depth can take place, because it is shown by the observed amount of precession, that the thickness of the earth's solid crust must at least be much greater than that quantity. This apparent discrepancy can only be removed, it would seem, by supposing *pressure* an antagonist cause with reference to heat considered as the cause of fluidity. This still requires the verification of experiment, but here assuming it to be true, it is easily seen how the crust of the earth may be generally solid to any depth, while portions of it may be necessarily fluid, from the removal of the superincumbent pressure by the formation over it of an *arch* or a *dome* in the manner above described.

XXIV. *Intelligence and Miscellaneous Articles.*

URINE OF THE ELEPHANT.

IT is shown by the experiments of MM. John and Vogel, that the urine of the elephant does not contain benzoic, but hippuric acid. M. Brandes has examined it and has obtained the same results, and his experiments prove that it contains a notable quantity of hippuric acid, and he procured it perfectly pure.

He found in this urine :

Hippuric acid combined with urea (hippurate of urea).

An azotized extractive matter, soluble in water and in alcohol.

Traces of a fatty matter and of a black resinous substance.

Hippurate of potash. Carbonate of lime.

Chloride of potassium. Phosphate of lime.

Sulphate of potash. Carbonate of magnesia.

Carbonate of ammonia. Mucus.

The urine of the elephant does not then form an exception to the constitution of herbivorous animals ; and M. Brandes concludes from his experiments, that the urea is combined with acid, as demonstrated by the researches of MM. Cap and O. Henry. The elephant's urine contains a considerable quantity of earthy salts.—*Journal de Chimie Médicale*, Dec. 1839.

DELVAUXINE.

Mr. Sandall has twice analysed a mineral named Delvauxine, which has the formula $\text{F}^2 \text{P} + 24 \text{H}$ attached to it, and finds its composition to be very nearly as follows :

Sesquioxide of iron	52.37
Water	40.19
Silica.....	3.95
Phosphoric acid	2.62
Lime.....	.87

100.00

The supposed composition denoted by the preceding formula is

Sesquioxide of iron	12.82
Water	69.23
Phosphoric acid	17.95

100.00

It will be seen, by reference to vol. xiv., p. 474, of the Lond. and Edinb. Phil. Mag., that Delvauxine, according to M. Dumont, is composed of

Peroxide of iron ..	30.30
Water.....	41.30
Silica	4.00
Phosphoric acid ..	13.95
Carbonate of lime	10.10

99.65

If the discoverer of the mineral will repeat his analysis, he wil

probably discover some source of error in his processes, or it may turn out that the mineral is only a variable mixture of the elements of which it is composed, and not a definite species.—R. P.

ON THE REDUCTION OF POTASSIO-CHLORIDE OF PLATINA.

M. Parisot observes that chloride of platina is employed in laboratories to ascertain the presence of potash and its salts in solution; and being consulted as to a method of easily separating the platina of the double chloride, he found that it might be effected by means of zinc.

The process consists in taking the precipitate of chloride of platina and potash, or the liquid which holds it in solution, and completely dissolving it in water, and the solution is then to have sulphuric added to it. Into this acid liquid a cylinder of zinc is to be immersed; decomposition of water occurs, attended with the evolution of hydrogen and the formation of sulphate of zinc; this remains in solution, while the platina is precipitated in the state of a black powder.

The reaction is maintained, with the disengagement of hydrogen, by adding a little sulphuric acid, until all the platina is precipitated, which is known to be the case when the liquor is perfectly colourless.

When the platina is reduced it is collected on a filter, washed with boiling water, then dried, and afterwards treated with a little hot hydrochloric acid, to deprive it of any zinc which it may retain, and lastly it is to be washed with boiling distilled water.—*Journ. de Chim. Méd.* Avril, 1840.

DR. BARRY ON THE CORPUSCLES OF THE BLOOD.

We are requested by Dr. Martin Barry to mention, that the rapid and incessant changes in the form of the blood-corpuscles observed by him in certain altered states, described in a paper read before the Royal Society, June 4, 1840, are caused, as he has subsequent reason to believe, by contiguous cilia.

ACECHLOR-PLATINA.

M. Zeise obtains this compound by triturating chloride of platina with a sufficient quantity of acetone to form a thin paste, and leaving the mixture in close vessels from 38 to 40 hours. During the solution of the chloride heat is given out, and an irritating vapour, mixed with hydrochloric acid, is evolved. A fluid, brownish, black compound, is at first formed, but the greater part becomes a crystalline mass in about 24 hours; when the fluid portion has been separated, the mass becomes of a yellow colour when treated with small portions of acetone on a filter. The mother liquor contains some acechlor-platina, which may be obtained by distilling it to dryness with the acetone which has been used in washing; the blackish mass obtained is to be treated on a filter with fresh portions of acetone.

The properties of acechlor-platina are, that when dry it is inodorous, has a metallic astringent taste, burns on the approach of a taper, with a greenish flame, and leaves metallic platina; when heated in a glass vessel by means of an oil-bath, it supports a heat of 383° Fahr. without emitting any odour and without apparent change; but when heated to about 397° , it begins to blacken and to yield an acid and penetrating odour. At 437° , in one experiment, the whole was converted into a black mass, with the disengagement of acid vapours, and a peculiar smell. After a heat gradually increased to 572° Fahr., it appeared to yield no further odour; and when heated to redness it gave only a slight acid smell. The residue is perfectly black before contact with the air, nor do any metallic particles afterwards appear in it. It does not fuse or swell up previously to decomposition.

Water dissolves but a small portion of acechlor-platina; the solution is at first yellow, but becomes brown after a few hours; the salt also, which does not dissolve, becomes brown by remaining under water. When heated with water it becomes first deep brown, and afterwards black, yielding some peculiar products; æther dissolves but little, alcohol rather more, especially when hot and the salt crystallizes unchanged; acetone dissolves much more; the solution is yellow, it probably takes up a thirtieth part when cold and a twentieth when boiling; the solution does not redden litmus unless water be present; concentrated hydrochloric acid does not act upon it without the assistance of heat; and the solution may be boiled without alteration. A solution of potash dissolves acechlor-platina completely; the colour is brown; when heated with ammonia, or in the dry way with potash, barytes or lime, its properties are changed. If copper be digested in a solution this compound is acetone; it is covered in a few hours with a black powder, on the addition of a little hydrochloric acid; in fact, this circumstance occurs immediately, and a little gas is evolved. Mercury acts similarly, but an amalgam is produced, and afterwards a black powder separates: when phosphorus is put into a solution of acechlor-platina in acetone an insoluble black powder is formed, containing platina, carbon and phosphorus; sulphur gives a soluble compound.

If a mixture of acetone and nitrate of silver, which is slightly opaque, be added to a solution of acechlor-platina in acetone, an abundant precipitate of a pure yellow colour is immediately formed; but in two or three minutes it becomes black, and the mixture remains long turbid; nitric acid produces no effect when added to a solution of acechlor-platina.

An aqueous solution of chloride of potassium or sodium dissolves acechlor-platina more abundantly than mere water; the solutions are yellow, and suffer no sensible change by ebullition, which indicates the formation of a double compound; but if this actually occurs, it is much less stable than that which results from the action of these chlorides on combustible chloride of platina (by alcohol), and they differ probably in other respects.

The mean of several experiments gave as the composition of this substance,

Platina	53·5883
Chlorine	19·1010
Carbon	19·4260
Hydrogen	2·8980
Oxygen.....	4·9867

100·

which, according to M. Zeise, are equivalent to

1 atom of platina.....	1233·260	53·6920
2 atoms of chlorine	442·650	19·2710
6 atoms of carbon	458·622	19·6660
10 atoms of hydrogen ..	62·398	2·7166
1 atom of oxygen	100·000	4·8537

99·6993

Ann. de Chim. et de Phys., t. lxxii.

NITRITES FORMED BY DIRECT COMBINATION.

M. J. Fritzche forms these salts by passing nitrous acid, procured from the action of nitric acid upon starch, into water containing finely divided oxide of lead; the oxide is quickly converted into a white mass, which, on continuing to pass the nitrous acid gas into it, completely dissolves. The deep yellow solution thus obtained, yields by evaporation with a gentle heat a considerable quantity of nitrite of lead in yellow silky scales. A very small quantity of nitrate is formed in this operation; there is, therefore, no doubt that nitrous acid can combine directly with bases.—*L'Institut*, No. 341.

METEOROLOGICAL OBSERVATIONS FOR JUNE, 1840.

Chiswick.—June 1. Very hot and dry. 2. Thunder storm, with rain in heavy showers. 3. Fine. 4. Overcast. 5. Drizzly. 6. Cloudless and hot: heavy rain at night. 7—11. Very fine. 12. Overcast and fine. 13—15. Very fine. 16. Hot and dry. 17. Fine: showery. 18. Showery in the morning: windy. 19. Slight rain. 20—24. Very fine. 25—29. Cloudy and fine. 30. Hazy: rain.

The mean temperature was within a fraction of the average for this month. The quantity of rain was moderate. Westerly winds were unusually prevalent. On the whole the weather may be considered as having been favourable.

Boston.—June 1. Fine: Therm. 78° one o'clock. 2. Cloudy: Therm. at noon 53°: rain P.M. 3, 4. Cloudy: rain P.M. 5, 6. Cloudy. 7. Fine: rain early A.M. 8. Fine. 9. Cloudy: rain P.M. 10, 11. Fine. 12. Cloudy: rain P.M. 13, 14. Fine. 15. Cloudy. 16. Fine. 17. Fine: rain early A.M. 18. Cloudy. 19. Rain. 20. Fine. 21. Cloudy: rain P.M. 22. Fine: rain early A.M.: rain P.M. 23. Fine: rain P.M. 24. Fine. 25. Cloudy: rain A.M. and P.M. 26. Fine. 27, 28, 29. Cloudy. 30. Fine.

Applegarth Manse, Dumfries-shire.—June 1, 2. Mild with occasional showers. 3. Fine day: bright sunshine. 4. Cloudy but dry. 5. Rain in the evening. 6. Rain in the morning. 7, 8. Very fine day. 9. Showery all day. 10. Fine day: rain early. 11. The same, but fair. 12. Wet all day. 13. Very fine day. 14. Wet greater part of the day: thunder. 15. Wet afternoon. 16, 17. Stormy and wet afternoon. 18. Showery, but calm. 19. Wet A.M.: cleared up P.M. 20. Showery A.M.: cleared and fine. 21. Showery all day. 22. Rain A.M. 23. The same: cleared and was fine. 24. Fair all day and cold. 25. Showery. 26. Drizzling all day. 27. Showery. 28, 29. Beautiful summer day. 30. Wet morning.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTSON; by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. VEALE at Boston, and by Mr. DUNBAR at Applegarth Manse, Dumfries-shire.

Days of Month. 1840. June.	Barometer.				Thermometer.						Wind.				Rain.			Dew point. Land: Roy. Soc. 9 a.m.
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.		Self-register. Fahr. 9 a.m.	Chiswick.		Dumfries-shire.		Land: Roy. Soc. 9 a.m.	Chiswick.	Dumfries-shire.	Boston.		
	Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Max.		Min.	Max.	Min.							
1.	30.212	30.195	29.50	29.88	29.85	66.7	83.0	55.7	84	50	59½	50	S.	SW.	W.	58
2.	29.806	29.981	29.808	29.75	29.73	64.8	82.7	60.5	65	45	59	48	NW.	NW.	NW.	60
3.	30.106	30.144	30.082	29.90	30.00	56.8	66.2	48.8	63	41	54	45	NW.	NW.	SW.	48
4.	30.198	30.168	30.044	29.59	29.98	55.7	61.0	48.3	66	51	52½	46	NW var.	W.	calm	50
5.	30.040	30.040	29.928	29.52	29.95	29.93	57.7	65.8	65	50	53	47	W.	calm	calm	54
6.	29.938	29.919	29.799	29.78	29.76	61.8	66.2	52.2	77	55	59	47	E.	SE.	SE.	54
7.	30.018	30.014	29.956	29.70	29.85	63.2	72.3	56.8	74	41	65	61½	SW var.	W.	S.	..	0.17	58
8.	30.072	30.045	29.949	29.45	29.78	63.7	73.8	52.5	75	57	67	40	SE.	calm	calm	53
9.	30.008	29.986	29.908	29.40	29.78	67.3	72.6	60.8	78	50	63	55	SE.	SW.	SW.	60
10.	30.014	30.016	29.993	29.34	29.79	64.8	74.4	59.5	77	43	67½	55	SE.	calm	calm	59
11.	30.100	30.082	29.974	29.42	29.82	64.3	71.7	52.8	74	59	66	45	S.	W.	SW.	55
12.	29.998	29.988	29.841	29.33	29.70	66.6	73.3	61.2	71	57	64	53	S.	calm	calm	60
13.	29.992	30.045	29.965	29.35	29.75	29.62	63.5	71.2	77	45	59	63	W.	N.	W.	..	1.32	57
14.	30.096	29.997	29.970	29.48	29.73	29.65	65.7	72.8	74	47	63	62	S.	N.	W.	59
15.	30.054	30.027	29.884	29.74	29.60	64.8	75.3	54.4	77	56	63	62	SSW.	SW.	SW.	59
16.	29.868	29.860	29.784	29.50	29.30	66.8	74.3	59.6	78	57	68	61	S.	SW.	SW.	61
17.	29.836	29.804	29.767	29.12	29.32	29.37	64.3	74.3	73	49	66	61½	S.	SW.	SW.	58
18.	29.870	29.945	29.858	29.19	29.50	29.60	61.8	71.6	70	49	61	45½	NW.	W.	W.	53
19.	29.970	29.988	29.895	29.35	29.42	29.59	61.7	67.7	62	45	57	49	S.	SW.	SW.	58
20.	30.150	30.201	30.103	29.50	29.82	30.00	61.2	72.6	75	52	60	47	W.	W.	W.	..	0.73	54
21.	30.198	30.193	29.917	29.60	29.80	29.70	65.0	69.8	77	53	61½	46½	S.	SW.	calm	56
22.	29.836	29.823	29.741	29.16	29.40	29.48	63.8	76.2	72	49	58	46½	SW.	W.	W.	55
23.	29.736	29.826	29.659	29.14	29.50	29.62	62.0	71.7	70	46	59	47½	SW.	W.	W.	52
24.	29.838	29.845	29.805	29.32	29.70	29.74	54.3	67.6	64	44	53	39	NW.	N.	N.	50
25.	30.010	30.004	29.958	29.39	29.78	29.86	57.4	60.6	64	47	57	47	NW.	N.	W.	48
26.	30.154	30.115	30.090	29.56	29.95	30.00	61.3	66.8	67	54	60	45	NW.	W.	W.	50
27.	30.196	30.146	30.056	29.60	30.00	29.97	63.7	65.4	74	57	61	45	NW.	NW.	W.	54
28.	30.092	30.045	30.022	29.52	29.99	29.94	64.8	70.4	73	51	62	46½	NW.	E.	NW.	55
29.	30.100	30.041	29.993	29.54	29.94	29.87	61.4	70.6	74	51	60	46½	S.	W.	calm	58
30.	30.018	29.970	29.893	29.37	29.74	29.74	63.3	73.0	69	47	68	55	S.	W.	W.	59
Mean.	30.017	30.015	29.918	29.68	29.415	29.761	62.6	71.2	71.96	49.93	61.5	61					1.38	Mean. 55.5

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[THIRD SERIES.]

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XXV. *Reply to that part of Mr. Weaver's Paper relative to the Mineral Structure of the South of Ireland, which has appeared successively in the Numbers of the Philosophical Magazine for April, May, and June, 1840. By RICHARD GRIFFITH, F.G.S. London and Dublin*.*

[With a Plate.]

HAVING read with much care Mr. Weaver's paper published in successive Numbers of the Philosophical Magazine, relative to the mineral structure of the south of Ireland, &c., in which he endeavours to support his own views† in opposition to mine, respecting the order of succession of the rocks, and the geological position to which each is referable, I think it incumbent on me briefly to reply to those views and statements, which are quite inconsistent with facts carefully observed by me, and represented in my large geological map, as well as in several sections, exhibiting what appears to me to be the true order of succession of the strata of the south of Ireland: some of these sections have been published in the Journal of the Geological Society of Dublin‡, and others in the Numbers of this Magazine for March last.

As the boundaries of the several rock districts as represented on the map, and their relative positions as exhibited in the sections, differ materially from those published by Mr. Weaver, they are considered by him to be imaginative compositions, and consequently unworthy of credit; but I hope to be able to prove that my map and sections are founded solely on a careful observation of facts, and in no case from hypothetical deductions.

* Communicated by the Author.

† See vol. v. Transactions of the Geological Society of London, second series.

‡ See vol. ii. part 1.

It is quite true that in the counties of Waterford, Cork and Kerry, I have made considerable changes in the colouring of my large geological map of Ireland, as compared with the small one appended to the Second Report of the Irish Railway Commissioners; but I should observe that, at the period of the publication of that map, and of the condensed Outline of the Geology of Ireland which accompanied it, I entertained doubts as to the accuracy of my views regarding the true position in geological sequence of some of the arenaceous and schistose strata of our southern districts. From the Wernerian character of my geological education, I found it almost impossible to conceive that quartz-rock and clayslate could be newer than the old red sandstone, or even belong to that series; and consequently when I found that the coarse conglomerates of the counties of Tipperary and Waterford, which rest *unconformably* on undoubted transition slate, alternated with, and were succeeded in an ascending order by red clayslate and red and gray quartz-rock, I was induced to refer the whole series to the transition class; but, judging from their unconformable position in regard to the older clayslate, I considered them to belong to a *newer transition series*, and described and coloured them as such.

Soon after the publication of the Railway Report I made a careful *re-examination* of our southern counties, and on comparing the well-characterized old red sandstones of the Slieve-naman and Galties mountains of the county of Tipperary, and of the Cahirconree or Slieve Meesh range of the county of Kerry, with the conglomerate series of the county of Waterford, I found them to be *identical in geological position*, and to present no difference in mineral character beyond a superior fineness in the grain of the upper members of the red slate series which occur in the valleys of the rivers Blackwater and Lee in the counties of Waterford and Cork. Under these circumstances it became evident that I should either obliterate the old red sandstone from my map, and class the whole with the transition series; or assuming the *unconformable position of the red conglomerate and slate* with regard to the older clayslates as an indication of a distinct period of formation, arrange them with that series of rock to which the name of old red sandstone has been given by British geologists.

In colouring the large geological map of Ireland, I adopted the latter principle, and thereby removed the anomaly which occurred in the small one, in which the conglomerate series of the mountains of Clare, Tipperary, and the north of Kerry were coloured as old red sandstone, while rocks occupying

the same geological position, and presenting the same mineral character and composition (as in the Monavoullagh and Knockmildown mountains, and other portions of the counties of Waterford, Cork and Kerry) were coloured as belonging to the upper portion of the transition series. The investigation which led to this change having been very extensive and minute, enabled me to make numerous corrections in the detail of the outlines of the rock districts throughout the southern counties, and I think it right firmly to assert that every one of those subdivisions to which Mr. Weaver has applied the term *gratuitous*, has been the result of careful observations laid down on the spot, on a map on a large scale. Whether the subdivisions of the strata I have made may or may not eventually be considered as indicative of the boundary of distinct *formations*, still they will be valuable as indicating the boundaries of different rocks presenting distinct mineral characters and fossil remains. Thus, in my map and sections I have endeavoured to represent with accuracy the relative positions of the several rocks as they occur in geological sequence, and when a more correct nomenclature shall be established, it can be applied to both without difficulty and without *altering a single line*.

Mr. Greenough, in his introductory memoir to the new edition of his geological map of England, has classed the old red sandstone with the graywacke formation, and has quoted from Dr. M'Culloch's *System of Geology*, the characters of the formation as exhibited in Scotland, which he thinks are equally just when applied to the west of England, and in my opinion correspond very accurately with those of Ireland, particularly of the south; but I still adhere to the propriety of separating these strata from the transition class on account of their *almost universal unconformability**, which forms a stronger line of distinction between the transition slate and the old red series than we find between the old red and the strata which succeed it, where no unconformability is observed, each suite, in the ascending series, succeeding the other in regular succession without any break, as far up at least as the new red sandstone; consequently, there is really more difficulty in determining the line between the old red sandstone and the carboniferous limestone series, than between it and the transition slate†.

* The only examples of conformability that I am acquainted with, occur in the west of the counties of Cork and Kerry. See paper by me in the *Lond. and Ed. Phil. Mag.* for March last, pp. 161—175

† The lower beds of the carboniferous limestone in many parts of Ireland, alternate with yellowish gray sandstone, which graduate into old red.

As to the term old red sandstone, I agree with most others in thinking it objectionable ; but if this rock occurs in similar positions and presents similar mineral characters in the west of England, in Scotland, and in Ireland, a local name, as "Devonian," cannot be considered suitable, and the provincial term *killas*, recommended by Mr. Greenough, though preferable, as not indicating any particular locality, does not appear to be applicable to a general series of rocks.

But I am forgetting the object of this communication, and must return to Mr. Weaver. In common with all geologists connected with Ireland, I feel much indebted to him for his laborious investigations in that country; and although I differ from him in many most material hinging points, still I fully admit that he has effected much that is accurate and valuable. Entertaining such feelings towards him, I regret to find that he has not entered upon the discussion of the differences between our respective geological labours with the fairness I should have expected, as, in making his strictures on my geological maps and papers, he has compared the explanation which accompanied the production of *my large geological map*, with the *small one* and the "Geological Outline" appended to the Railway Report. As my large geological map was exhibited at the meeting of the British Association at Newcastle in 1838, at the time I read my paper on the geological structure of the south of Ireland, and as in that paper I mentioned the change that I had made in relation to the old red sandstone series of the southern counties, Mr. Weaver in considering that paper should have referred to the map which it was meant to illustrate, and not to the small geological map, the discrepancies in the colouring of which, as already expressed, it was intended to correct. Similar observations are applicable to the papers, also referred to by Mr. Weaver, read before the Geological Societies of London and Dublin, in May and June, 1839, both of which, as mentioned in the commencement of those papers, were communicated on the presentation of a copy of my large geological map, published in March of the same year.

Instead of comparing these several papers, all of which refer to the geological structure of the south of Ireland, with the large geological map, Mr. Weaver has compared them with the small map, and the "Geological Outline" which accompanied it; we need not therefore be surprised at his observing differences and incongruities, and through them attempting to throw discredit on everything which I have done.

It is true that Mr. Weaver has introduced notes in several

places referring to the large geological map, in the first of which he states, "that his paper was drawn up before he had seen another geological map on a large scale, published in 1839;" and he observes, "that as the alterations that have been made in the new map do not *materially* interfere with the course of his argument, which in the first instance bears on the map attached to the outline, he has left the text unaltered." Now in his text Mr. Weaver does not confine his observations to the outline and small geological map, but refers equally to the three papers read by me before the British Association, and the Geological Societies of London and Dublin, the whole being compared with the small geological map, though at the period of the publication of his paper in April 1840, my large geological map had been in the possession of the London Geological Society for eleven months, and was to be seen exhibited in Mr. Gardner's, in Regent Street, for upwards of a year; so that it does appear extraordinary, that Mr. Weaver, who in the commencement of his paper alludes to my several communications above-mentioned, in each of which my large geological map is mentioned, should not have seen it when he drew up his paper for the Philosophical Magazine.

With this explanation I will conclude my observations on the subject of the discrepancies noticed by Mr. Weaver between my small geological map, and the sections and papers intended to illustrate the large one.

In comparing Mr. Weaver's geological map of the south of Ireland with mine (the large one), it must be admitted that the differences *are very considerable*, both in the great scale and in the detail. The latter is not to be wondered at, as I possessed much better maps, and had many opportunities of examination in almost every locality; but in regard to the great features, some explanation appears to be necessary. Mr. Weaver considers the whole of the schistose strata situated to the south of the river Suire in the county of Waterford, and thence to the south coast of the county of Cork, to belong to the transition series, on the northern part of which, particularly on the summits of the Knockmildown and Monavoullagh mountains, strata belonging to the old red sandstone series are superimposed in an unconformable position; and that the limestone of the valleys of the rivers Suire and Blackwater belong to the carboniferous series, while that of the valleys of the rivers Bride, Lee, &c. is transition, and alternates with the transition slate.

According to my view, the transition slate within the above-mentioned limits is confined to the dark gray slate district

south of the conglomerate of the valley of the Suire and east of that of the Monavoullagh mountains; while the conglomerate of these mountains rests unconformably on the slate strata to the north and east, and *dipping* to the southward, forms the substratum or base of the whole of the arenaceous, quartzose and schistose strata to the southward; and consequently, as these conglomerate beds are admitted by Mr. Weaver to belong to the old red sandstone series, the strata which rest upon them cannot belong to the transition class. I am also decidedly of opinion that the limestones of the valleys of the Suire, the Blackwater, the Bride, and the Lee, all occur in the same geological position, being placed on the top of the series, and that as the limestone of the valleys of the Suire and Blackwater are admitted by Mr. Weaver to be carboniferous, all the others must likewise belong to the same series.

In proof of the accuracy of these views, I formed with great care a section passing through this district nearly in a north and south direction; and if this section exhibits a correct representation of the relative positions of the several rocks, Mr. Weaver's views must be erroneous.

This section was exhibited at Newcastle in 1838; at the Geological Society of London, in May, 1839, in illustration of my paper; and subsequently at the Geological Society of Dublin, in June, 1839, and has since been published in the *Journal of that Society*.

I shall now proceed to consider Mr. Weaver's objections to my sections, both as to the principle of their construction and their accuracy.

He objects to the principle of making the scale of the height much greater than the scale of length, as he observes that "without considerable labour it leads to inaccuracy, as such sections, unless done correctly, instead of conveying precise information, tend rather to mislead the judgement." I quite agree to the position that an inaccurate section will tend to mislead; but if a section be made with care, and the relative positions of the rocks be accurately laid down according to the respective scales, no error can arise, though the angle of the dips will be necessarily increased.

The direct advantage to be derived from making the scale of height greater than the scale of length is, that supposing the scale of 8 to 1 be taken, the length of the paper on which the work is laid down is but $\frac{1}{8}$ th of the length which it would be, if equal scales of length and height were adopted; and in long sections this is most important, by bringing the whole subject under the eye at one time, instead, as must otherwise be the case, of having it in a long roll, or different portions

placed one beneath the other, by which means the continuity is broken.

Suppose the scale of height adopted for a section of a mountainous district be 2000 feet to an inch, and the distance between the termini be 100 miles, on equal scales, the length of such a section would be 22 feet; while by adopting a scale of 8 to 1 for the height, the length would be but 2 feet 9 in., and if carefully constructed the latter would give as clear a representation of the structure of the country as the other.

In regard to the second point, namely, the *accuracy* of my sections, Mr. Weaver observes, "that they appear to him in many respects drawn rather according to the conceptions of their author, than the occurrences in nature." This is certainly not complimentary; but as my sections through the same district differ in many important points from those made by Mr. Weaver, I cannot be surprised at the opinion: my consolation is, that should I prove mine to be correct, his opinion of my work will be applicable to his own.

The part of my section near the east coast to which Mr. Weaver objects, is from the valley of the river Suire to the south coast in the county of Cork; and first, as he observes, respecting "that part which lies between the valley of the Suire and the vale of Dungarvan, which latter extends westward to the Blackwater."

As far southward as the conglomerate of the Monavoullagh mountains, which rests unconformably on the old clayslate of the county of Waterford, there is no difference of opinion between Mr. Weaver and me; we both consider the limestone trough of the Suire to be carboniferous limestone, and the red slate and conglomerate to be the old red sandstone; but Mr. Weaver states that I am incorrect in making the old clayslate to the south of the Suire dip north; he says, the dip is to the *south*. Now I have examined the stratification with great care, both previously and subsequently to the publication of his paper, and I must state, that in the line of my section from the hill of Carrick southward by Millvale to Rathcormuck, the general dip of the *cleavage* in the old slate series is to the south, but the dip of the *strata*, as determined by the sedimentary lines, is to the northward; consequently Mr. Weaver in this case must have mistaken *cleavage* for *stratification*. It should be observed in this place, that although the slate strata dip towards the north, they are not conformable with the overlying conglomerate of the valley of the Suire, the latter dipping to the north at an angle of 80° , while the ends of the slate beds, where the junction is clearly visible, abut obliquely against the conglomerate, and dip 30° to the west of north at angles varying from 35° to 60° .

We may next consider the Monavoullagh conglomerate, which in his map Mr. Weaver shows as an insulated tract, surrounded by transition slate; I, on the contrary, show in my section a precipitous escarpment to the north, but a gradual declivity towards the *south* and south-west. If Mr. Weaver be correct, his conglomerate, which he represents as a mountain cap, must rest unconformably on the transition slate on the south as well as on the north side; now, I positively assert that such is not the fact, but that the alternating series of red clayslate and conglomerate, which presents so striking an escarpment on the north face of the mountain range, *forms the lower part of the red slate series*, and that proceeding from the summit of Crotty's rock to the southward we regularly ascend in the series, till at length in approaching the Blackwater near Lismore, we meet the yellow sandstone with calamites, which in so many localities alternates with the carboniferous limestone, that I have been induced to consider it rather as the lowest member of the carboniferous limestone than the upper part of the old red sandstone series*. Now if I am right in this position, what becomes of Mr. Weaver's mountain cap, or of the occurrence of the old transition slate to the southward of the Monavoullagh mountains? As an additional proof of the correctness of my views on the subject, I have made a careful examination of the district *within the last month*, and have prepared a plan which has been constructed with great care, and which, I think, will set the matter at rest.

The plan represents that portion of the county of Waterford extending from Ballyvoil Head in a western direction, along the valley of the Ballyvoil river to Gloundogan, a distance of four miles, and southward from thence to Dungarvan. In this district we have the unconformable outgoing of the conglomerate of the Monavoullagh mountains, extending in an eastern direction *without interruption* to the coast at Ballyvoil Head. Now Mr. Weaver states† in his paper, "that there is no apparent connexion whatever between the horizontal sandstone and conglomerate of the Monavoullagh range and those beds of conglomerate and red slate of the coast which continue eastward from Dungarvan in several separate discontinuous bands interstratified with the other transition rocks." In opposition to this opinion, I beg to state, that the alternating strata of red clayslate and conglomerate of the Monavoullagh escarpment, does extend uninterruptedly from Crotty's rock to the shore at Ballyvoil Head, where they rest equally in an unconformable position on the transition slate.

* See Lond. and Ed. Phil. Mag. for March, 1840, p. 173.

† See Lond. and Ed. Phil. Mag. for April, 1840, p. 279.

The contact of the conglomerate with the transition slate is clearly visible at Island Hubbock, about a quarter of a mile north of Ballyvoil Head, and their unconformability is unquestionable, as the conglomerate rests upon the upturned ends of the slate, and strongly adheres to them. In illustration of this fact, I beg to refer to the section No. 1, which gives an accurate representation of the red slate and conglomerate strata as they occur, resting unconformably on the dark gray transition slate, from Island Hubbock westwards. The succession is as follows:

1. Unconformable base composed of alternations of blackish gray and reddish-gray clayslate with chlorite slate and gray quartz-rock, the blackish-gray clayslate greatly predominating; dip of strata from 10° to 20° east of north, at angles varying from 60° to 75° .

2. Alternations of reddish-gray and red conglomerate, reddish-gray compact sandstone, and dark-red slate, the conglomerate predominating; dip of strata about 35° west of south, at angles varying from 60° to 80° . The thickness of these alternating strata is about 300 feet.

3. Dark reddish-gray clayslate, reddish-gray quartz-rock, and sandstone, the slate predominating: thickness about 300 feet.

4. Dark red micaceous sandstone, and red clayslate, with occasional beds of conglomerate, the sandstone predominating; dip 25° west of south at angles varying from 60° to 85° : thickness about 750 feet.

5. Dark-red slate, red quartz-rock and sandstone, the slate predominating: thickness about 300 feet.

6. Brownish-red quartzose rock, red clayslate, and yellowish-gray sandstone, the sandstones predominating: dip 20° west of south, on an average at an angle of 65° : thickness about 600 feet.

To the south of the yellowish-gray sandstone and red slate the strata are concealed from view by diluvial matter for a distance of about half a mile, beyond which alternations of dark-gray carboniferous slate and limestone are visible, both of which contain abundance of fossils belonging to the carboniferous limestone series.

I also give a section of the strata as they occur, from the unconformable contact of the red slate and conglomerate with the transition slate at Gloundolgan, to the coast at Ballynacourty in the harbour of Dungarvan, in which the succession of the strata between the lower conglomeritic base, the yellow sandstone, the carboniferous slate, and superincumbent carboniferous limestone are clearly shown.

Now if we compare these sections with that originally

published by me as extending southward from Crotty's rock in the Monavoullagh mountains to Lismore, we find exactly the same suite interposed between the conglomerate base and the carboniferous limestone of the valley of the river Blackwater.

It is true, that the distance between Crotty's rock and Lismore, is greater than between Gloundolgan and Knocknagranny, in section No. 2, or between Ballyvoil Head and Ballyvoil bridge, in section No. 1; but it will be observed by referring to the plan, that the conglomerate and red slate at the coast dip to the south at an angle of 70° ; while at Gloundolgan, four miles west of Ballyvoil Head, they dip south at an angle of 45° ; further to the westward the dip is south at an angle of 15° ; and as we approach the summit of the Monavoullagh mountains at Crotty's rock, the strata effect a nearly horizontal position. Owing to this circumstance, though the section is complete at Ballyvoil Head in the horizontal distance of one mile and a half, yet between Crotty's rock and Lismore the same suite occupies a horizontal distance of eighteen miles.

Having stated these facts, I think it unnecessary to do more than observe that Mr. Weaver's argument in proof of the impossibility of the red slate and conglomerate at Crotty's rock composing a part of the same series with the red slate and conglomerate of Ballyvoil Head, namely, *the nearly horizontal position* of the one, and the highly inclined angle of the strata of the other, is untenable; and I confess I am surprised at such an argument being used, as every practical geologist must be aware of its weakness.

The northern part of Mr. Weaver's section between the Suire and the Blackwater does not take the same line as mine, as his crosses over the Knockmildown mountains, and mine over the Monavoullagh range situated to the east; but to enable me to test the accuracy of his section, I have made one nearly in the same line. (See section, No. 3. in the plate.) If we compare this section with Mr. Weaver's, nothing can be more dissimilar. Mr. Weaver's section represents a base of graywacke slate, which supports unconformably a cap of old red sandstone; but according to my section, it is evident that the entire mountain range belongs to the old red sandstone series. The anticlinal axis is exposed to view in the valley of the river Ownashad, at Corrignagour, three miles north of Lismore: it consists of alternations of dark-red slate and dark-red quartzose rock, the slate predominating. These strata are succeeded both on the north and south sides of the axis by beds of brownish-red quartz-rock, red sandstone, and red clayslate, occasionally associated with fine-grained conglomerate, a thin bed of which appears at the surface on the south

side of the axis at Shruggh, and on the north side at Crooked Bridge on the road from Lismore to Clogheen. In the lower part of this series the quartz-rock and sandstone predominate, but in the upper part the red clayslate is the prevailing rock.

Descending the hill on the north side towards Clogheen, and on the south towards Lismore, we find the last-mentioned rocks succeeded by alternations of gray sandstone and red clayslate, beyond which we have the limestone series alternating in the commencement with dark-gray clayslate. There is much diluvial matter at the base of the declivity on the north side, and in consequence the yellow sandstone with calamites is not visible there; although to the north of Lismore, at Reaf, and in other parts of both valleys, it may be observed associated with the dark-gray slate, which alternates with the lower beds of the carboniferous limestone.

If we compare the succession of rocks which form the stratification of the Knockmildown mountains, as above described, with those that occur at Ballyvoil Head and Gloundolgan, it would appear that the lowest visible rocks of the Knockmildown range belong to the *upper part* of the old red sandstone suite, which will account for the absence of the thick beds of conglomerate which abound in the lower part of the series in the Monavoullagh range, as at Crotty's rock, Gloundolgan and Ballyvoil Head.

In my paper printed in the *Journal of the Geological Society of Dublin**, I have entered so fully into my reasons for considering that the limestones of the valleys of the Suire, the Blackwater, the Bride, the Lee, &c., occupy the same geological position, both by the order of succession of the strata and by fossils, that I do not think it necessary to discuss the subject a second time; and conceiving that I have shown that the red slates and conglomerates which overlie the conglomeritic base at Crotty's rock, at Gloundolgan, and at Ballyvoil Head, occupy the entire space between the dark-gray transition slate and the carboniferous limestone, I think I have substantiated my case, and shown that the strata situated to the south of the unconformable junction with the transition slate of the county of Waterford, do belong to a *newer series*, to which, for reasons already given, I have applied the name of old red sandstone in my large geological map, and in the papers referable to it. I shall, however, reply to one statement of Mr. Weaver's in regard to the stratification of the ridge interposed between the valley of the river Blackwater and the Bride, which, he observes†, "are said by Mr. Griffith to partake of a similar composition to that of the northern

* See vol. ii. part 1.

† See Lond. and Ed. Phil. Mag. for April, 1840, p. 228.

side of the Blackwater near Lismore, &c., forming in the centre of the ridge an anticlinal axis." He further observes, "The anticlinal axis I have not seen, the dip which I observed *being throughout to the south,*" &c.

Now I must assert that the anticlinal axis *does exist*, and is clearly visible in the section of the strata of this ridge, which is exposed to view on the west bank of the river Blackwater, which in this locality takes a southern course, and cuts through the ridge between Killahally, opposite to Dromana Castle on the north, and Camphire on the south, as may be clearly seen by reference to my large geological map. The anticlinal axis is visible nearly in the centre between Camphire and Killahally, the strata at the axis and on either side consisting of alternations of red quartzose rock and red clayslate, which are succeeded both on the north and south by alternations of gray quartz-rock and red slate, those to the north dipping north at angles varying from 60° to 85° , and those to the south dipping at angles varying from 35° to 40° . These strata are succeeded on the south side by yellow sandstone containing calamites, and lastly, by alternations of limestone and black clayslate. On the north side the yellow sandstone is not visible, owing to a covering of diluvial matter, but the limestone alternating with black clayslate is visible immediately to the north of Killahally, dipping to the north at an angle of 60° ; consequently, there can be no doubt of the identity of the limestone of the valley of the river Blackwater at Lismore, and thence to Dungarvan, with that of the Bride at Tallow, Camphire, &c.

It is to be observed, that the limestone of the valley of the Blackwater immediately to the south of Lismore, as represented in my section, published in the Journal of the Geological Society of Dublin, dips to the south at a very high angle; but fortunately this is not *universally* the case, as at Killahally, three miles south-east of Lismore, the strata, as above-stated, dip to the north, which in regard to the trough-shape of the limestone, proves by observation what I had previously supposed to be the case by induction.

In like manner I am prepared to follow my section in detail from the valley of the Bride to the south coast at Cork Head, and to show that at each point the *strata do actually dip* in the direction exhibited on the section; but I must remark in reference to the section published in the Journal of the Geological Society of Dublin, that in one point, namely, on the west side of Cork harbour, the lithographer has represented the beds of the carboniferous limestone as abutting against the subjacent carboniferous slate, while in the original drawing, as in nature, they are conformable.

I shall now make a few remarks with regard to Mr. Weaver's section as compared with my own, and particularly with reference to the position of the limestone of the several troughs in which it occurs.

We agree with respect to the limestone of the valleys of the Suire and the Blackwater, both considering it to be carboniferous; but what are the geological circumstances under which these limestones occur? They both rest conformably on the red slate and conglomerate series, which in the valley of the Suire Mr. Weaver calls *old red sandstone*, but in the valley of the Blackwater *transition slate*. In respect to the latter, I have shown that he is mistaken by means of my original section, and the sections from Ballyvoil Head and Gloundogan; but Mr. Weaver in his section exhibits the carboniferous limestone of the Blackwater at *Lismore* in the form of a trough, the north side dipping to the south, and the south side to the north, which is not the fact; as in that locality, as already mentioned, the carboniferous strata dip to the south on both sides of the valley, on the north side at an angle of 30° , and on the south at an angle of about 60° .

If Mr. Weaver had carefully examined the dips of the strata, he must have observed this fact; and, following the principle he has adopted in other places, he should have included this limestone in his transition suite; but knowing this was not the fact, from its connexion with the great limestone field of Ireland, he has represented this rock at Lismore as a trough having reverse dips on the opposite sides of the valley.

We next come to the limestone of the valley of the Bride at Tallow, which Mr. Weaver in his section truly represents as a trough having dips in opposite directions; but this limestone, though undoubtedly the same in geological position, in lithological character, and in fossils, as the limestone of the Blackwater, he makes transition, though from its position and true trough shape, he must have considered it to rest on the top of the red slate series which lie beneath it on the north side of the valley dipping to the south, and on the south side to the north.

I shall now make a few observations respecting the extensive district represented by me as carboniferous slate, which occupies the greater portion of the south of the county of Cork, and which Mr. Weaver has included in his transition district. In geological position it rests upon the red slate and conglomerate series; near its commencement it is usually interstratified with gray or yellowish-gray sandstone and arenaceous slate, which frequently contain calamites, and in some cases, as at the Old Head of Kinsale, where the strata are

unusually compact, the sandstone passes into quartz-rock, but it still exhibits the calamites.

In many localities the carboniferous slate includes beds of limestone, which, together with the slate, contains fossils similar to those which occur in the lower beds of the carboniferous limestone. This is the case at Blackball Head*, on the north-western extremity of Bantry bay; also at Brickeen island, near Killarney; at Kenmare†; at Clonea Castle on the coast of the county of Waterford, south-west of Ballyvoil Head; at Goat island in Ardmore bay on the same coast; at Kilnamack, near Knocklofty bridge; on the south side of the valley of the Suire above Clonmel, and many other localities.

The following are a few of the fossils which occur in the beds of carboniferous slate which are interstratified with the limestone at the following different localities:

Fossils which have been discovered in the carboniferous slate beneath and interstratified with the admitted carboniferous limestone at the undermentioned localities.			Fossils which have been discovered in the carboniferous slate beneath and interstratified with the limestone of Killarney and Cork Harbour, supposed by Mr. Weaver to belong to the transition series.	
Ballinacourty, Dungarvan Bay.	Clonea Castle, Dungarvan.	Kilnamack, near Knocklofty Bridge, valley of the Suire.	East of Brickeen Bridge, near Killarney.	Below the Cork limestone at Renniskiddy, Shanbally, and Killingly.
Calamopora tumida
Retepora tenuifila
.....	Retepora laxa	Ret. membranacea	Ret. membranacea
.....	Ret. membranacea
Turbinolia fungites	Turb. fungites	Turb. fungites	Turb. fungites
Amplexus Sowerbii	Amplexus Sowerbii
Producta lobata	Producta lobata	P. depressa	Producta lobata	P. depressa
.....	P. depressa	P. depressa
.....	P. scabricula
.....	P. aurita	P. aurita
.....	P. martini
.....	Leptæna lata	Leptæna lata, (Sow.)
.....	Spirifera attenuata	Sp. attenuata	Sp. attenuata
.....	Sp. bisulcata	Sp. bisulcata	Spirifera bisulcata	Sp. bisulcata
.....	Sp. lineata	Sp. lineata
.....	Sp. resupinata	Sp. resupinata
Spirifera glabra	Sp. glabra
.....	Sp. symmetrica
.....	Sp. crenistria	Sp. crenistria	Sp. crenistria
.....	Sp. semicircularis
.....	Sp. filaria	Sp. filaria	Sp. filaria
.....	Sp. laminosa
.....	Sp. arachnoidea	Sp. arachnoidea
.....	Sp. imbricata	Sp. cuspidata
Pleurorhynchus elongatus
Pl. minax
Nucula tumida	Nucula tumida
Nu. undulata
.....	Asaphus gemmuliferus	As. gemmuliferus
.....	As. truncatulus
.....	As. quadrilimbus

* In the dark-gray clayslate of Blackball Head I found *Spirifer semicircularis*, Phillips.

† *Retepora membranacea* occurs in the carboniferous slate at Roughty bridge above Kenmare, where it rests conformably on yellow sandstone containing calamites.

I shall next advert to Mr. Weaver's observations respecting my section which extends from Brandon bay, at the extremity of the peninsula of Dingle in the county of Kerry, in an eastern direction across the Cahirconree or Slieve Meesh range of mountains, thence traversing the limestone valley of Castle island, and terminating in the great millstone grit of Munster*.

Of this section, Mr. Weaver observes, "That the portion which more immediately claims attention, is that which extends from the summit of the old red sandstone of the Slieve Meesh range, to the carboniferous limestone of Castle island. The former is represented as constituting nearly a cap or sheet, formed on an inclined plane, from west to east, the strata corresponding and succeeding each other in that direction to the junction with the carboniferous limestone." Mr. Weaver further observes, that he "knows of no such arrangement; on the contrary, the strata of the old red sandstone are accumulated to a great depth, and certainly, in some quarters, at least to the level of the sea, being disposed in a gently arched form from north to south."

In the latter observation Mr. Weaver is perfectly correct: in fact, the Slieve Meesh or Cahirconree range may be compared to a semicone, having its base to the west and apex to the east; the western base presents a precipitous escarpment, the lower region of which is occupied by highly-inclined strata, consisting of dark gray clayslate, which on the outer edges alternates with purple clayslate; the nearly upright ends of these strata are covered by a series of unconformable beds of compact red sandstone and red conglomerate, alternating with coarse red slate; near the summit, these strata present a nearly horizontal arrangement, in a north and south direction, but they *dip to the eastward at a moderate angle*. On approaching the declivities of the cone, both to the north and south, the conglomerate strata dip rapidly, on the one side, towards Tralee bay, and on the other, towards the bay of Castlemaine. From the summit, the eastern dip is continued towards the apex of the cone at Currans; but the lower bed, which rests on the ends of the transition slate, does not continue to form the surface, but dipping more rapidly to the eastward than the ridge of the hill, it is succeeded by a number of beds of conglomerates and coarse slate, each cropping out to the westward. At the eastern extremity of the range, or the apex of the cone, the upper portion of the red slate and conglomerate series is succeeded in a conformable position by beds of fine-grained yellowish-gray sandstone

* See Journal of the Geological Society of Dublin, vol. ii., part 1.

of the carboniferous series, some of which contain calamites and many obscure casts of bivalves, one of which was named by Mr. Sowerby as the *Avicula modiolaris* *. The upper beds of the sandstone alternate with a dark-gray, and occasionally blueish-gray quartzose-rock, and they are succeeded by dark-gray clayslate, alternating with carboniferous limestone. These strata, at Riversville quarry, which I have lately visited, dip to the east at an angle of 15° . It was here that Mr. Weaver could discover traces only of the graywacke formation; now the upper part of the quarry just mentioned contains thin beds of carboniferous limestone; and immediately to the south and east we have large quarries of that rock partaking of the same strike and dip as the schistose beds beneath it, which rest conformably on the strata belonging to the old red sandstone series of the Slieve Meesh range. How Mr. Weaver can consider beds in such a situation to belong to the transition series, I cannot understand; for, as to his idea of there being a protrusion † of graywacke from beneath the old red sandstone, it cannot be sustained, there being no reverse dip; on the contrary, the yellow sandstone and dark-gray slate rest conformably on the old red slate, and are succeeded by strata of limestone having the same strike and dip ‡.

In regard to fossils in the yellow sandstone and carboniferous slate of this locality, as I mentioned in my paper which has been quoted by Mr. Weaver, they contain numerous imperfect casts of *Producta*, *Spirifera*, *Terebratula*, *Crinoidea*, and *Retepora*; but though I lately sought, with much care, I did not discover any varieties of *Orthis* or *Favosites*, supposed by Mr. Weaver to occur there.

The foregoing description of the structure of the Cahircree, or Slieve Meesh range, is similar to that contained in my paper just alluded to, which has been verified by recent observations; but as Mr. Weaver was not convinced of the inaccuracy of his views respecting the carboniferous slate at the eastern base of the Slieve Meesh range, which, notwithstanding my section and description, he still considered to belong to the graywacke series, I do not expect that what I now repeat will have the effect of changing his opinion. But it should be observed, that this carboniferous slate, which un-

* Journal of the Geological Society of Dublin, vol. ii., part 1.

† See Lond. and Ed. Phil. Mag. for April, p. 291.

‡ In my section already alluded to, the lithographer did not make an accurate copy of the original, and has made the limestone strata to rest unconformably upon the carboniferous slate, while in nature these strata are conformable.

derlies and alternates with the undoubted carboniferous limestone of the valley of Castle island, is precisely similar to the carboniferous slate which underlies and is interstratified with the lower beds of the carboniferous limestone at Clonea Castle, on the east coast of Waterford, near Dungarvan; it is likewise similar to the rock which alternates with the limestone of Cork harbour, of Killarney, of Kenmare, and of many other localities in the south of Ireland; consequently, as Mr. Weaver persists in the opinion that the limestone of Cork harbour, of Killarney, of Kenmare, &c., belongs to the transition series, it would be fatal to his argument to class the carboniferous slate of Clonea Castle, or of Currens, with the carboniferous limestone series. But I will observe, that in the localities just mentioned, beds of undoubted carboniferous limestone alternate with slate, precisely similar in fossils, as well as in lithological character, to that of Cork harbour, &c.

I do not think it necessary to pursue this argument further than to observe, that in endeavouring to form a distinction between the admitted carboniferous limestone of the valley of the river Laune, and that of Killarney, and also between the admitted carboniferous limestone of the valley of the Blackwater, below Mallow, and that westward of Clonmeen Castle in the same valley, Mr. Weaver has involved himself in an untenable dilemma. There is no difference in geological position, in mineral character, or in fossils, between the limestone of Killarney, and that of the valley of the Laune between Beaufort bridge and Killorglin, which are all contained in the same valley, and all repose on the same base; and a similar statement may be made in regard to the limestone of the valley of the river Blackwater, above Clonmeen Castle, and that at and below Mallow; yet Mr. Weaver considers the limestone of Killarney and that above Clonmeen Castle to be transition, and that below Beaufort bridge and Mallow to be carboniferous.

After what has been said, I hardly think it necessary to reply to the observations contained in the postscript to Mr. Weaver's paper*, in which he endeavours to show that I am incorrect in considering the conglomerate and red sandstone of the Gap of Dunloe, and that to the south of the Lower Lake of Killarney generally, as identical with the red sandstone, the conglomerate, and red slate of the Cahirconree or Slieve Meesh range.

* Published in the Philosophical Magazine for June last. [L. and E. Phil. Mag. vol. xvi. p. 471.]

It should be observed, that the red conglomerates and red slates of the district of Killarney are situated on the *south side* of the carboniferous limestone valley of Castlemaine, while those of Cahirconree are on the *north side*; that the upper beds of both graduate into the carboniferous limestone series, at Currens on the north side, and at Brickeen island in the Lower Lake of Killarney on the south side of the valley. The unconformability of the conglomerate beds with the transition series on Cahirconree, and their conformability on McGillacuddy's Reeks, the Purple Mountain, &c., is no proof that the rocks are not identical, as, in England, the old red sandstone graduates both into the Silurian and mountain limestone series.

In respect to the fault described by me, the occurrence of which is *doubted* by Mr. Weaver, I shall observe, that it is clearly visible at the Gap of Dunloe and at Brickeen island; and I will assert, that the positions of the old red sandstone strata on one side, and the chloritic rocks on the other, in both those places, are as clearly indicative of a fault as any I have ever seen. On the west side of the Gap of Dunloe, we have a perpendicular cliff upwards of 200 feet in height, which is traversed by a nearly upright cut or crack about 20 feet in breadth. On the south side of this cut we find strata of dark-green chloritic quartz-rock dipping to the south at an angle of 30° , while the strata on the north side dip to the west at an angle of 10° , and are composed of rather fine-grained conglomerate and a red quartzose-rock or compact sandstone identical with that which lies beneath the red conglomerate of Cahirconree. I am of opinion that these appearances do prove that there has been a fault.

Figure No. 4 in the plate is an accurate representation of the fault as above described.

Similar observations are applicable to the appearances at Brickeen island. There also, the strata on the south side of the fault consist of green chloritic rock, having rather a slaty structure, which dip to the south; while on the north side, we have in succession, red quartzose-sandstone and red slate, red limestone, and yellowish-green slate containing calamites, abutting obliquely against the chloritic rock on the north side.

Towards the conclusion of his paper, Mr. Weaver observes, "Proceeding now to the Dingle peninsula, the succession given also by Mr. Griffith from *north to south*, namely, from Brandon bay to Foylaturrive, is as follows: 1st, dark gray clayslate," &c. The above sentence is a *misquotation* from my paper. My words are, "If we make a section across the

Dingle peninsula, from Foylaturrive on the *south* to Brandon bay on the *north*, we find that the strata consist of a base of dark blackish-gray clayslate," &c. Now by reversing the points, Mr. Weaver has made the dark gray clayslate to occur on the north side of the peninsula, namely, at Brandon bay, while it really occurs on the south: the misquotation was doubtless unintentional, but as Mr. Weaver's argument was founded on this misconception, it is unnecessary to reply to it.

In concluding my observations, I cannot avoid expressing my regret that Mr. Weaver was not present either at the meeting of the British Association at Newcastle, or at the Geological Society of London, when I communicated my views relative to the geological structure of the south of Ireland; for, *viva voce* discussion tends more to clear up geological differences than lengthened written descriptions; and where both parties are in search of truth there is little difficulty in attaining it.

Dublin, July 8, 1840.

XXVI. *Memoir on the Law of Substitutions, and the Theory of Chemical Types.* By M. DUMAS.

[Continued from vol. xvi. p. 505, and concluded.]

Organic Radicals.

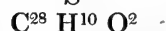
FOR some years organic chemistry has so frequently used what we call *organic radicals*, that it will appear singular to see, if not their existence, at least the reality of the absolute function which they have been made to play, here put in doubt.

We know that by the term organic radicals we mean to designate certain compound bodies which might fulfil their functions in the manner of simple bodies, and which might enter, as they do, and following the same laws, into combination with the various bodies of nature.

If by organic radicals, bodies analogous to cyanogen, to amidogen, to the oxalic or benzoic radical be intended, there is no doubt that there, in fact, compound bodies perform the function of simple bodies, like those analogous to them in mineral chemistry, the oxide of carbon, sulphuric acid, the binoxide of azote, and nitrous vapour.

But if by the term organic radicals we must, as M. Berzelius wishes, designate certain invariable compounds which would fulfil the function of the metals, the theory of types, while admitting their concurrence, cannot allow their permanency.

Thus to fix our ideas, in the theory of types the essence of bitter almonds is a type in which we can substitute for an equivalent of hydrogen an equivalent of chlorine, of bromine, of iodine, of oxygen, or of amidogen, without the type being altered,



But whilst admitting that an element might be substituted for the system $\text{C}^{28} \text{H}^{10} \text{O}^2$, the theory of types does not consider it as an invariable group. It believes that hydrogen may be taken from that group, that chlorine may take its place, or that it may be made to undergo every other modification without its fundamental nature being altered by it.

In a word, by a reciprocity easy to foresee, and which to receive all its development would require a detail of formulæ which I cannot enter upon here, we arrive at the conclusion, that in the same manner that it is possible in an organic compound to substitute sulphuric acid, which fulfils the same function for hydrogen, so we may in certain organic matters substitute a simple body for a group of molecules representing a compound body.

To say that nitrous vapour takes the place of hydrogen in nitrobenzine, is the same as if we said that in æther potassium may take the place of æthyle.

But we must not conclude from this that æthyle is a permanent, immutable (*immuable*), unchangeable compound, for experience proves the contrary. Only by losing some hydrogen and gaining chlorine everything leads us to suppose that it preserves its character, as does the æther, of which it makes a part.

But I admit that in a given type there are certain compound groups for which simple bodies may be substituted, and which in so far would deserve the name of radicals. They fulfil the same function as ammonium, which takes the place of the potassium in alum, for example.

Thus I cannot consider these groups as immutable bodies, for experience has pronounced the contrary, and every theory which would absolutely rest on this basis would go too far.

Amongst the researches which contribute the most to mo-

dify the opinion on the function of the organic radicals, we should cite in the first place the important observations of M. Laurent on the essence of bitter almonds, and those not less remarkable of M. Piria on the hydruret of salicyl^e *. To resume ; nothing hinders me from retaining the name of organic radicals for certain molecular groups capable of being substituted for elementary bodies which may reciprocally be substituted for them, but these groups may in their turn be modified by substitution, like the other bodies which do not perform this function.

I had a memoir of M. Gehhardt put into my hands, but too late for me to make use of it in the present notice, in which these questions are examined in a manner which appeared to me very worthy the attention of chemists.

Nomenclature.—Amongst the questions which are presented to us as being the immediate consequence of the point of view which we have just set forth, there is one which deserves particular attention ; it has relation to the principle itself of our chemical nomenclature, and to the modifications which the progress of the science has led us to make it undergo.

At the memorable period when the French Academicians, under the influence of the immortal discoveries of Lavoisier, conceived and unfolded the project of a reform in the old chemical nomenclature, they grounded themselves upon the view which Lavoisier himself had just established, that is, upon the existence of those undecomposed substances which were recognized as the material elements of all bodies.

Seeing that by the aid of these elements all the bodies of nature could be produced, that in associating them two and two binary bodies were formed, that in combining these one with another salts were produced, and that in combining these salts in their turn double salts were obtained, the nomenclature had to follow the philosophical principle in all its developments. It required that the names of the elements should be set forth in those of the binary compounds, that they should reappear in the names of simple salts, in those of double salts, &c.

What strikes us in the chemistry of Lavoisier, and in the nomenclature which was the consequence and the expression of it, is the antagonism of the elements which combine to form the binary compounds ; it is the antagonism of the acids and of the bases which combine to form salts ; it is the antagonism of the salts which combine to form double salts, &c.

* [See Lond. and Edinb. Phil. Mag., vol. xvi. p. 210, 211.]

The chemistry of Lavoisier and its nomenclature seemed then to have foreseen and prepared the electro-chemical theory, which has had nothing else to do than to call one of these antagonist bodies the positive element, and the other the negative element.

But let us not lose sight of the great discovery of Lavoisier; it is the discovery of the elements. This is the fundamental principle by which he revived chemistry and natural philosophy. Not a truth of this order is discovered without leaving its impress on all our thoughts; and for the same reason that Lavoisier had established that all the bodies of nature might be formed by means of some elements, he would be led to define the compound bodies by the elements which compose them, and there, in fact, is the principle that our nomenclature has appropriated.

Now not only is the nomenclature of Lavoisier no longer sufficient for us, but it expresses a system of ideas quite contrary to that which we seek to cause to prevail.

It is no longer sufficient for us, because in organic chemistry thousands of combinations are produced with three or four elements, and consequently those could not lend themselves to name all the compounds which result from them.

It is positively contrary to the system of ideas explained above, in this, that it derives the notion of the bodies from the nature of its elements, whilst the latter have only what may be called a secondary interest in the classification.

Each type must have a name, and this name should be found in the numerous modifications which it may undergo, so that it should never disappear so long as the type itself is not destroyed.

It is on this principle that I have already formed the following names: *acetic acid* and *chloracetic acid*, *ether* and *chlor-ether*, *olefiant gas* and *chlorolefiant gas*; names, the object of which is to set forth, as may be seen, the permanency of the types, notwithstanding the intervention of chlorine in the compounds.

The theory of types views these bodies in some degree as *casts from the same mould, with different materials*. It would have the nomenclature always recal their fundamental molecular arrangement, and that it should be put in the first line, whilst the nomenclature of Lavoisier applies itself to the material, brings out the nature of it, and places this notion first.

The theory of types tells you, here is alum of chromium;

the nomenclature of Lavoisier sees in it sulphate of potassa and of chromium under the form of alum.

Alum is a type; all the alums are cast in the same mould; their form is what the theory of types would set forth especially; that which essentially defines each of them. It acts as an artist, who in seeing the statues consisting of different materials cast from the same mould, will say to you, "Here is the Venus of Milo in brass, in lead, in plaster." The artistic type strikes him before he dreams of the material, and he will never think of saying that he is about to show you brass, plaster, or bronze in the form of the Venus of Milo.

An entire reform of the organic nomenclature and of some parts of the mineral nomenclature, appears to me, then, both urgent and possible.

Electro-chemical theory.—We just now saw how the principle of dualism, introduced by the chemistry of Lavoisier in the definition of every chemical combination, was favourable to the conception of what is called the electro-chemical theory. We have also understood how the theory of molecular types swerves from this order of ideas, for it does not suppose two antagonist elements present in the bodies, acting as would two masses endowed with different electricities, and held in combinations by the mutual action of these two electricities.

Does a chemical combination constitute a simple edifice or a double monument? this is the question. In the theory of types, the formulæ combine, and are written without attending to the reduction (*dédoubler*) of each body into two others. In the electro-chemical theory they combine, and are written in such a manner as always to paint to the mind these two principal divisions of the edifice which they represent.

This is the manner in which the theory of types has been driven to separate itself from the electro-chemical theory, or rather that in which this latter has been led to combat the other from its first appearance. The question, however, is given in the clearest way in the following letter from M. de la Rive. The skilful Genevese philosopher, whose name will always be united with the history of electro-chemistry, wrote to me on the 25th of October last (1839):—

"I have read your researches on *substitutions* with very great interest. They interested me the more as I have been occupied for more than a year upon a rather large work on the electro-chemical theories. I dare not, I must confess, go as far as you; and without believing in the theory of Berzelius such as he has presented it, yet I cannot help thinking that there is something well-founded in the table of the relative chemical

powers of bodies. Now, that hydrogen can perform the function of chlorine exactly, is what I can hardly admit.

"Allow me to ask you if chemists are not rather easy (*facile*) when they group their symbols in every way. There is in this facility of permutation something which does not completely satisfy us physicists, and which appears to lend itself rather too complacently to all combinations. Is there not something arbitrary in the manner in which chemists make these choices? To attack the electro-chemical theory you group your formulæ in a certain manner; immediately to defend this theory M. Berzelius groups them in another manner; where is the law of nature?"*

I shall be pardoned for quoting this letter; it depicts the opinions of philosophers upon questions still new to many minds, and in all cases very obscure to those persons who have not followed them step by step in their development.

Those who have taken a part in the experimental researches of which we are speaking, know well that the electro-chemical theory guided my first studies, that I professed and admitted it for a long time on the faith of its inventors. They also know,

* [It is proper to add here, that M. de la Rive, in the *Bibliothèque Universelle* for February 1840, p. 193, after reciting the passage extracted from his letter by M. Dumas, as above, makes the following remarks on the subject of it and the comments of M. Dumas:—

"In writing these lines to M. Dumas, I sought, as he himself remarked, to satisfy myself concerning a question which becomes every day more obscure. On the one hand, we cannot help recognizing that in organic chemistry, especially the electro-chemical theory, or rather the chemistry which connects the development of electricity with the play of the affinities with which it is always accompanied, has on its side powerful arguments, even when we do not admit on this point all the views of M. Berzelius. On the other hand, there are certainly some phænomena, especially in organic chemistry, in which the function of the same elements in the formation of compounds seems to change its nature in a manner so complete and so extraordinary, that we cannot admit of their possessing a previous predisposition to conduct themselves in such and such a chemical manner, or what comes to the same thing, an absolute electro-chemical power. Would not the result of this seem to be that the electro-negative or electro-positive properties do not previously exist in bodies? That they do not exist until the bodies are presented to each other, and that from that time instead of being *absolute* they are *relative*, that is to say, depend for the same body on the relations which exist between its own nature and that of other bodies in the presence of which it is found? This point of view can only be thoroughly examined by means of direct experiments. I shall return to it when I have finished bringing together a number of facts sufficiently considerable for its justification, if, as I presume, I find it to be founded. I shall be glad to try thus to reconcile, at least in part, the function which M. Berzelius attributes to electricity in the chemical phænomena with the very remarkable laws at which M. Dumas seems to have arrived."]

that it is the force of circumstances, that it is a clear and convincing experience, the production of chloracetic acid, which has led me to admit that hydrogen and chlorine perform the same function in certain compounds. I constructed my formula according to pure chemical experience, my mind being free and disengaged from every view of general theory.

But to admit that chlorine may take the place of hydrogen and perform the same function, was to separate oneself from the chemists who would explain all the phenomena of combinations by means of what is called the electro-chemical theory. I understood it thus, and I found it necessary to explain myself in a direct manner. Besides, how could we believe that this consequence would have escaped the penetration of M. Berzelius, when we see all the value he attaches to giving an immediate explanation, according to the electro-chemical theory, of each of the facts which daily enrich the theory of substitutions, and when we are able to appreciate the high talent he displays in the combination of the formulæ which his theory requires?

It was not necessary to say to M. Berzelius, that in the views of electro-chemistry the *nature* of elementary particles should determine the fundamental properties of bodies, whilst in the theory of substitutions it is from the *situation* of these particles that the properties are especially derived.

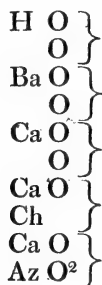
We have, however, on this head decisive facts in the domain of mineral chemistry itself. Thus oxygen, sulphur, selenium, tellurium, chromium, iron, manganese, magnesium, and hydrogen constitute a series of bodies capable of taking each other's places, without the form or essential properties of the compounds being changed by it. Thus M. Berzelius attributes to the nature of the elements the function (*rôle*) which I attribute to their position: this is the ground of our respective opinions: let us now come to the point where they separate in practice.

Amongst the consequences of the electro-chemical theory, one of the most immediate consists in the necessity of viewing all chemical compounds as binary bodies. We must always find in each of them the positive particle and the negative particle, or the whole of the particles to which these two functions are attributed. Never was view more capable of shackling the progress of organic chemistry. All the difficulties which we have felt for some years in the inquiry concerning the fundamental formulæ of bodies, the discussions, the misconceptions, the errors, spring from prepossessions which this view had given rise to in our minds.

Some examples will make these two points of view easy of comprehension.

Carbon can combine with oxygen, and thus form carbonic oxide and carbonic acid. In its turn carbonic oxide combines with chlorine, and produces the acid gas discovered by Dr. John Davy. The electro-chemical theory should see in this last an acid chloride of oxide of carbon. The theory of types views it, on the contrary, as carbonic acid, in which for half the oxygen chlorine is substituted. Thus the bodies CO^2 , COCh , CS^2 are modifications of the same type*.

Oxygenated water is a type, and one of the neatest and best-defined (*nets*) that chemistry possesses. Supply the place of the hydrogen by a metal, and you will have the binoxides of calcium, barium, strontium, and in general the simple (*singuliers*) oxides. For these, substitute in its turn for half the oxygen, chlorine, as is the case in chloro-carbonic acid, and you will produce the decolorating chlorides. Thus oxygenated water, the simple oxides and the decolorating chlorides, belong to the same type, to which must also be added the compounds which binoxide of azote forms with the alkaline oxides, so, for example, that we may have the following series:



These compounds of oxides and of chlorine have received all kinds of definitions in the electro-chemical system. Chlorides of oxides, of chlorites, of hypochlorites, have been made of them, as people were guided by the pretended necessity of always putting together in the formula of a compound two antagonist bodies, the positive and the negative.

This is precisely the character of the differences at the pre-

* Here is what I said of phosgene gas in 1828:—"It is easy to see that chloro-carbonic acid corresponds to carbonic acid itself. In fact, in all its combinations one volume of chlorine takes the place of one half-volume of oxygen; it is then as if the oxide of carbon had been changed into acid, by substituting for the half-volume of oxygen which it was necessary to add, a volume of chlorine."—See my *Traité de Chimie*, vol. i. p. 513.

sent time between the electro-chemical school and the school of molecular types.

If acetic acid is deprived of all its hydrogen, and chlorine substituted for that hydrogen, we say that acetic acid and chloracetic acid possess the same molecular arrangement, and that they should possess the same general actions so long as their molecule is not destroyed. Urged by the convenient principles, the electro-chemical system, M. Berzelius, on the contrary, makes of chloracetic acid a separate body, in which he arranges the elements into two groups, which he supposes to be combined with each other. In his opinion the chloracetic acid becomes a compound of oxalic acid and chloride of carbon, a formula which is in no way justified, for chloracetic acid treated with potassa should give chloride of potassium and oxalate of potassa, whilst, according to my experience, it really gives carbonic acid and chloroform.

It is just the same thing as when it was said that the bin-oxide of calcium and the chloride of lime belonged to the same type, that which the interesting and decisive experiments of M. Millon have so well proved; whilst M. Berzelius, relying on ingenious researches, was induced to assert that lime when uniting with chlorine gave rise to a chlorite.

If M. Malaguti takes two equivalents of chlorine from æther, the theory of types foresees and explains that in their place there must have entered into the new product two equivalents of chlorine. It sees æther in the new product, as to the molecular constitution and the fundamental properties.

But M. Berzelius, on the contrary, as might have been supposed, disposes the elements of this new body, and those of the products of which it makes a part, in such a way as to make them constitute binary compounds, which according to these formulæ would possess actions quite opposed to those which have been recognized by M. Malaguti.

In all cases in which the theory of substitutions and the theory of types see *single molecules* losing some of their elements and substituting others for them, without the edifice being modified in its form or its exterior actions, the electro-chemical theory reduces (*dédouble*) these same molecules, solely we must say to find those *two antagonist groups* which it afterwards supposes combined, in virtue of their reciprocal electrical action.

Thus in my opinion the electro-chemical theory has been drawn out of the circle which experience traces for us, when it would have explained the new facts of organic chemistry. But is it to be asserted that the electrical properties of bodies are

without influence on chemical phænomena? Unquestionably not: only it must be agreed, that it is at the moment when the combinations are made, at the moment when they are destroyed, that the function (*rôle*) of electricity may be observed.

But when the elementary molecules have taken their equilibrium, we know not any longer how to define the influence that their electric properties may exercise, and no one has put forth views on this subject which agree with experience.

I have then been induced to declare that the facts which I have just discovered were irreconcilable with the electro-chemical theory of M. Berzelius, who considers hydrogen as always positive and chlorine always negative, whilst we see them supply each other's place, and perform the same function.

But I am far from denying, on that account, that the chemical and electrical forces may be the same, and there is no reason to take up the defence of the general function of electricity in chemical phænomena, when it is simply a particular electro-chemical theory which is under discussion. What I wished to say, what I said, is, that when we have endeavoured to represent the electric state of the combined molecules, pure hypotheses have been attained without any result for the science.

When, on the contrary, as has been done so happily by our colleague M. Becquerel, an endeavour has been made to take advantage of this electricity which shows itself at the moment of chemical combinations or decompositions, results the most important and the most fruitful have been obtained.

It is in this class of facts that the beautiful discoveries of Davy may be classed, those which M. Becquerel pursues with so much perseverance and success; in fact, the experimental law with which Mr. Faraday himself more recently enriched chemical philosophy.

All the discoveries of these great physicists have reference to the phænomena of the chemical action, and are quite independent of the views which they may have expressed on the function of electricity in compound bodies.

In the course of this memoir I have several times made use of the actions (*réactions*) of bodies, as being the only method quite proper for unfolding their real nature. There is notwithstanding an objection in the experiments themselves, to which I have often referred the reader, thus:—

A chemist who, without knowing the origin of it, had had to study the body $C^8 H^6 Ch^4 O$, seeing that under the influence of potassa this body is changed into chloride of potassium and acetic acid, would certainly have seen in it either a chlo-

ride of the acetic radical, or acetic acid, in which for a portion of the oxygen chlorine had been substituted ; yet this body is nothing but the chlorinated æther of M. Malaguti.

Just so, when I was occupied with the study of chloroform $C^4 H^2 Ch H^6$, the manner in which it acts with the potassa, the formation of the chloride of potassium, and of the formic acid which result from it, led me to consider it as being formic acid, $C^4 H^2 O^3$, in which equivalent quantities of chlorine supplied the place of the oxygen ; yet M. Regnault has lately shown that chloroform is nothing but some hydrochloric æther of methylene, in which chlorine has supplied the place of a portion of the hydrogen, the body $C^4 H^6 Ch^2$ being changed by this substitution into $C^4 H^2 Ch^6$.

The result of these examples, which might be multiplied, would be, that the actions of bodies are not a faithful guide, for they lead us to refer to acetic acid, a body derived from æther, and to formic acid, a body which represents hydrochloric æther of methylene. But in looking nearer, we see, in fact,

Æther	$C^8 H^{10} O$
Chlorinated æther	$C^8 H^6 O$
	Ch^4
Acetic acid	$C^8 H^6 O$
	O^2 ,

belong really to the same molecular grouping, and that in saying that chlorinated æther is derived from æther, and that it produces acetic acid, nothing really contradictory has been affirmed.

On the other hand,

Methylic æther	$C^4 H^6 O$
Formic acid	$C^4 H^2 O$
	O^2
Chloro-methylic æther	$C^4 H^6 Ch^2$
Chloroform	$C^4 H^2 Ch^2$
	Ch^4 ,

constitute bodies of the same molecular grouping, so that chloroform may be viewed as anhydrous formic acid, or as bichlorinated chloro-methylic æther, without these two ways of regarding it at all contradicting each other.

The result of this is, that chemical actions, without possessing the absolute character which has often been given to them, deserve a confidence which may have been momentarily shaken, but which a profound examination again establishes in its true place in our minds.

In fact, we have admitted that substitutions may unveil the molecular grouping of bodies by furnishing a set of equa-

tions of condition which the general formula ought to satisfy. Now it is evident that the metamorphosing actions are often nothing but means for operating substitutions, by taking advantage of affinities more complicated than those which are made use of in the ordinary substitutions.

It is therefore more than ever requisite to apply to the study of the actions of bodies, and not to trouble ourselves about the distance which often separates the point from which we started from that which we reach; for it may well happen, that these two points, so unlike in their properties, are really united to each other by the theory of substitutions, and belong to the same molecular grouping.

The law of substitutions expresses, then, a simple experimental relation; it is limited to the expression of a relation often observed between the hydrogen lost and the chlorine absorbed, by a hydrogenated body submitted to the action of chlorine. This law establishes only, that if the substance loses 1, 2, 3 equivalents of hydrogen, it will gain 1, 2, 3 equivalents of chlorine; but it does not explain this fact.

The theory of types goes further; it explains what the law of substitutions is content to determine. It considers organic bodies as being formed of particles, which may be displaced and have their places supplied by others without the body being destroyed, so to speak. In the cases above-quoted, the molecule of acetic acid, that of æther, may lose hydrogen and take chlorine, without ceasing to constitute an acid or basic molecule, formed of the same number of equivalents and endowed with the same number of fundamental properties.

It is then because, that on pain of being destroyed, the molecule of acetic acid must take an equivalent of chlorine to stand for the equivalent of hydrogen which it loses, that this substitution, this *remplacement* is effected. Thus it is that the theory of types explains the law of substitutions.

The substitution of one element for another, equivalent for equivalent, is the effect; the preservation of the type is the cause. The organic molecule, the organic type, constitute an edifice, in which a course (*assise*) of hydrogen can have its place supplied by a course of chlorine, of bromine, or of oxygen, without the exterior relations of that edifice being thereby modified. But it is necessary, when the course of hydrogen is taken away, to put something in its place; if not, the edifice crumbles or is transformed.

The law of substitutions was hardly put forth before it became the subject of severe criticisms in Germany, to which I thought it useless to reply. If this law was just, it was for

experience to teach us; if it was false, it was experience which would pronounce its falsity. In all cases it was necessary to leave time to determine its place in science.

The theory of types was scarcely published when the same criticisms were reproduced, at least by M. Berzelius; and notwithstanding all my devotion to the interests of the science, I would again have left to time and experience the care of pronouncing on these debates.

But when I reflected, it seemed quite evident to me, that as a consequence of the researches of organic chemistry, general chemistry had reached one of those periods of crisis, when everyone owes to science the testimony of his convictions.

We cannot conceal from ourselves that two systems of ideas are before us:—one, which is supported by all the authority of the past, the rights acquired by quiet possession now for nearly a century, the tacit assent of a great number of chemists, and which reckons amongst its defenders and at their head, a philosopher illustrious amongst the most illustrious, M. Berzelius; the other, which consists in asserting that the bodies formed of the same number of chemical equivalents placed in the same manner, belong to the same molecular type, and often to the same chemical type.

This latter attributes to the number and arrangement of the particles an influence of the first order, which in the ideas of the received chemistry belongs especially to the nature of those particles. The law of substitutions would be the experimental demonstration of this new system, and would have led some of its partizans to adopt it. I do not claim its invention, for it does but reproduce and give precision to, under a more general form, opinions which are to be found in the writings of great chemists, and particularly MM. Robiquet, Mitscherlich, Liebig, Laurent, Persoz, Couerbe, &c. It is precisely this coincidence between the numerous facts, to the discovery of which the law of substitutions has led, and the opinions already known relative to the influence of certain pre-existing molecular arrangements, that has given me the confidence necessary for their adoption in my turn when I proposed the admission of organic types.

Here we have, then, before us two systems: one which attributes the principal agency to the nature of the elements, the other which reserves it for the number and arrangement of the equivalents.

Pushed to an extreme, each of them in my judgement would be found to lead to an absurdity. Regulated by experience and kept by it within prudent limits, each of them must take a large share in the explanation of chemical phæ-

nomena; and to explain by a last word the meaning which I attach to their respective functions, I shall say that in chemistry the nature of the molecules, their weight, their form and their situation, must each exercise a real influence on the properties of bodies.

It is the influence of the nature of molecules that Lavoisier has so well defined, it is that of their weight which Berzelius has characterized by his immortal labours. It might be said that the discoveries of Mitscherlich relate to the influence of their form, and the future will prove whether the present labours of the French chemists are destined to give us the key to the function which belongs to their position.

We subjoin to the preceding memoir by M. Dumas, a translation of an extract of a letter from M. Baudrimont, published in the *Comptes Rendus*, for March 16.

"M. Dumas says, that the law of substitutions, and the theory of chemical types, are unconcerned in the reclamations of M. Baudrimont, who does not admit them. This requires an explanation from me.

"I cannot admit M. Dumas's law of substitutions; first, because it has not the character of a physical law; secondly, because it is but the strict expression of an order of facts, much more extended than M. Dumas supposes; *but I admit chemical substitutions; for substitution is only one of the modes by which bodies may enter into combination.*

"I should without doubt do more than M. Dumas in saying to the Academy: *Chemical compounds are produced, either by direct combination or by displacement, or by substitution, or lastly, by several of these modes united. Substitution may be non-equivalent, equivalent, isotypic, isorhythmic, or isomorphic*; let me be pardoned this neologism. But this formula, which is true, has not the character of a law; it is but the general expression of facts which are within the knowledge of all men, ever so little versed in chemistry; for chemical substitutions have been known ever since we arrived at the knowledge that one metal can precipitate another, taking its place in a saline solution; ever since hydrogen was first obtained by displacing it by iron or zinc in the pretended sulphuric and chlorhydric acids diluted with water; ever since we knew that chlorine displaces bromine and iodine; ever since we knew isomorphism by substitution; and ever since M. Beudant made more than a thousand applications of them to the calculation of the composition of minerals.... I admit, then, chemical substitution; but I repudiate the pretended law of M. Dumas, for the reasons I have just set forth.

“As to what relates to chemical types, which M. Dumas says I do not admit, I dare hope that the Academy, and all enlightened men will not participate in this opinion; for it will without doubt be allowed that he who first classed chemical types, must necessarily have admitted them, even before M. Dumas had acquired any notion of them, as his memoir appears to show, since he makes this notion only go back to his experiments on chloracetic acid, the discovery of which is posterior to my thesis.”

M. Dumas replies, that his memoir is conceived in such terms, that it should have spared the Academy all the reclamations of which it has been the subject. In a historical note which he intends soon communicating to the Academy, he will show in what the views which are represented as identical differ, and to whom belongs the discovery of each of the principal points of the theory.

XXVII. *On the Ferrosesquicyanuret of Potassium. By*
ALFRED SMEE, Esq., Surgeon.*

THE action of chlorine upon the ferrocyanate of potassium is a subject of much interest to the chemist, and has not been examined to any extent in this country. It therefore has been my endeavour to investigate this action carefully, and to see under what circumstances the change from the ferrocyanate into the ferrosesquicyanuret takes place; and the methods which are here detailed to obtain this latter salt uncontaminated with impurities, will be found free from the difficulties and uncertainties attending on the present mode of preparing it.

When a current of chlorine is passed through a solution of ferrocyanate of potassa, or an aqueous solution of that gas is added to it in certain quantities, the persalts of iron are not precipitated. This solution has no smell of chlorine, and is changed from a yellow colour to a dark red, and deposits on evaporation red crystals. A similar change takes place when bromine is added to the ferrocyanate, and in both cases the weight of the entire red mass is equal to that of the yellow ferrocyanate, plus the weight of the chlorine or bromine used, but minus the quantity of water which the yellow crystals are known to contain. This indicates, first, that the red crystals are anhydrous; and secondly, that the chlorine or bromine is actually absorbed by the salt. The former fact is con-

* Read before the Royal Society, June 18, 1840; and now communicated by the Author.

firmed by heating the red precipitate in a test tube, when no water is given off; and the latter fact is also proved by the evolution of chlorine or bromine, on the addition of two or three drops of strong heated sulphuric acid to a few grains of red salt.

When heated alcohol is added to this red mass a small portion is dissolved, which is again deposited when the spirit is evaporated. This salt by its characters is known to be either the bromide or the chloride of potassium. By this method the red ferrocyanate of potassa, which is insoluble in alcohol, becomes purified; but this is a troublesome and expensive process, as the bromide or chloride is but little soluble in the spirit, and therefore a large quantity must be used.

About half an equivalent of chlorine or bromine is required to effect this change, and great care must be employed to prevent excess of these substances, as they are apt to react upon a portion of the salt. The liquid in this case contains Prussian blue dissolved, which materially discolours the salts, and it can only be precipitated from the solution by the addition of neutral salts, as sulphate of soda, which renders the red ferrocyanate impure. In a similar manner, chloride of soda, as might be expected, forms the red ferrocyanate of potassa.

From the foregoing details a knowledge is obtained of the action of chlorine and bromine upon the ferrocyanate, for we have seen that chloride and bromide of potassium is formed, and that one half an equivalent of these substances is necessary for this change. Now it is manifest that half an equivalent of potassium is removed from the ferrocyanate, so that the new salt, instead of consisting of iron one equivalent, potassium two equivalents, cyanogen three equivalents, contains iron one equivalent, potassium one and a half equivalent, cyanogen three equivalents; and therefore it is rightly named the *ferrosesquicyanuret of potassium*: that half an equivalent of potassium has been removed from the salt, two or three experiments have verified.

The acids as a class will not effect a similar change, because as they combine not with potassium but with potassa, water must be decomposed, the oxygen uniting with the metal, and the hydrogen passing to the ferrocyanate, forming hydroferrocyanic acid.

A question naturally arises whether the potassium may not be removed from the ferrocyanuret by other processes, and we are led to try the action of the anions, and of these I attempted to add oxygen to the salts by the use of nitric acid. This acid, when added in small quantities to the yellow ferrocyanate, acts as the other acids by liberating hydroferro-

cyanic acid, which is speedily decomposed into a pale blueish cyanuret of iron. When, however, further additions of this acid are made, the potassium takes oxygen, forms potassa, deutoxide of nitrogen is evolved, and the solution becomes dark coloured. This liquor, when neutralized with potassa, is found to give no precipitate with the persalts of iron, but forms Prussian blue with the protosalts of that metal. The rapidity of this change depends upon the heat of the solution, for when warm the effect takes place immediately, whilst on the contrary, two or three days are required at a low temperature. When evaporated, a large quantity of nitrate of potassa is deposited; and lastly some red crystals are formed. When acid is more used, the ferrocyanate is totally decomposed; the black mass which is the result has at first a sweet, but afterwards leaves a disagreeable metallic taste upon the palate. This process can never be used advantageously to form the ferrosesquicyanuret, from the quantity of acid which is required, the degree of nicety which must be employed to effect the change, and the impurity of the salt when obtained.

The next highly oxygenated acid which we have to examine is the iodic; this when added to ferrocyanate of potash becomes decomposed, the oxygen passes to the potassium to form potassa, free iodine is evolved, and the potassa passes to another portion of iodic acid, and is precipitated as the iodate of potassa. The free iodine can be readily removed by agitation with a little æther, and in this way a tolerably pure ferrosesquicyanuret of potassium can be extemporaneously obtained, for the solution contains but little iodate of potassa from its insolubility*.

Chloric acid operates in the same way as iodic acid, but is more difficult of decomposition, and it requires the action of heat before the smell of chlorine is exhaled and the red ferrocyanate formed.

If chlorate of potassa be added to the ferrocyanate, and dilute sulphuric acid be dropped into the solution, red ferrocyanate of potash will also be formed.

Bromic acid will not act upon the ferrocyanate with the production of the ferrosesquicyanuret, but acts as other acids in forming Prussian blue.

A great variety of other oxyacids have been tried, but none were found to part with their oxygen.

When a large quantity of peroxide of manganese in fine

* This elegant process can be employed with advantage when a small quantity of the salt is suddenly wanted, as it scarcely requires a minute to effect.

powder is added to a solution of the ferrocyanate of potash, and the mixture digested for a considerable time, the ferrocyanate becomes converted into the ferrosesquicyanuret, and on evaporation crystals of the most beautiful ruby red are obtained. The salt thus procured appears to be very pure.

If a little dilute sulphuric acid be added to the solution in conjunction with the peroxide of manganese, the action takes place more quickly, but sulphate of potassa is formed, which is a great disadvantage.

The last process in which nascent oxygen contributes to the formation of ferrosesquicyanuret of potassium, is, perhaps, one of the most elegant, efficient, and simple processes in the whole range of chemistry. This mode I was induced to follow from the consideration, that as nascent oxygen effects a change of the yellow to the red ferrocyanate of potassa, a similar change must be produced by its being subjected to a galvanic current. Accordingly some solution of the salt was placed in a tube bent like a syphon, and at the bottom a piece of tow was thrust, in order that a separation might so far be effected, that the solution on one side could not readily pass to the solution on the other. Having thus completed the arrangement, a galvanic circuit was passed through the fluid; when at the cathode, hydrogen was evolved, and at the anode no oxygen, on the contrary, was given off, but the solution became of a dark colour. The dark solution was found to precipitate only the protosalts of iron, and on evaporation deposited red crystals of the ferrosesquicyanuret, but at the cathode potash was discovered. The rationale of this change may be deduced from circumstances attending slight alterations of arrangement; for if on the zinc side of the bent tube a saturated solution of the ferrocyanate be placed, and on the platinum side distilled water, and then the galvanic circuit be completed, potash will appear at the platinode, and red ferrocyanate at the zincode. On the contrary, if the distilled water is placed at the zinc side and the ferrocyanate at the platinum side, potash is left at the platinode, whilst at the zincode no red ferrocyanate is found, but a substance which does not redden litmus paper, and which speedily decomposes into Prussian blue; this is probably ferrocyanogen. Thus it appears that one equivalent of the yellow ferrocyanate is decomposed, the free potash travels one way and the hydroferrocyanic acid the other; the oxygen unites with the hydrogen of the acid and sets ferrocyanogen at liberty; this again unites with an equivalent of ferrocyanuret of potassium to form the ferrosesquicyanuret.

Various other attempts were made to form the red ferrocyanate by oxygen, such as heating it with nitrate of potassa, but the mixture exploded at a temperature below redness.

When a mixture of powdered ferrocyanate and peroxide of manganese were heated together no ferrosesquicyanuret was formed. Several other oxides, as those of mercury, silver, tin, iron, &c. &c., were digested with ferrocyanate of potassa, but none that were tried, except the peroxide of manganese, formed the red ferrocyanate; many of them were converted into cyanurets.

A current of oxygen gas passed through the solution of the salt produces no alteration, showing that the gas must be in a nascent state to cause the change.

The next substance we have to examine is phosphorus, and its action is somewhat remarkable; for little or no change is effected by the addition of an alcoholic or ætherial solution of phosphorus. When a piece of phosphorus is also placed in a solution of the ferrocyanate, or when phosphorus is heated with powdered ferrocyanate, the sesquicyanuret is not produced; but if a stick of phosphorus is placed in a bottle containing a solution of the salt, and only a portion of it is covered with the liquor, the phosphorus gradually burns away, the solution becomes sour and red, and ceases to precipitate the persalts of iron. This change takes place with a rapidity exactly proportionate to the wasting of the phosphorus; for if the temperature is below 45° , but little action takes place, but above 60° the reddening is very speedily produced. The red solution is not to be tested with the salt of iron whilst it is acid, for in that case a copious greenish-white precipitate is produced of phosphate of iron; but after it has been neutralized with potassa a solution of baryta is to be added, to throw down the phosphate, and a drop of dilute sulphuric acid may then be added to remove any excess of baryta.

The solution will now be found not to precipitate persalts of iron, but, on the contrary, a large quantity of Prussian blue is produced with the protosalts. The actual combustion of the phosphorus seems essential to this change; for if the water in which phosphorus has been allowed to burn, be added to the solution of the ferrocyanate, a similar change will not be produced. The cause of this change appears paradoxical, for phosphorus has in other instances a deoxidizing agency, so that a piece placed in a solution of either gold, silver, platinum, or copper, has the metal precipitated upon it. Perhaps it depends upon decomposition of water and the formation of phosphuretted hydrogen; for a narrow bottle, to which air has but limited access, is more favourable to the change

than a wide vessel. If this explanation is correct, the action of phosphorus must be classed with the other oxygenating substances, for oxygen, and not phosphorus, removes the potassium*.

No mode of abstracting the half equivalent of potassium by sulphur is known, for if half an equivalent of sulphur be heated with powdered ferrocyanuret, the ferrosesquicyanuret is not produced, and the alcoholic or terebinthine solution of sulphur, added to a solution of the ferrocyanuret, also failed to produce this change. Even nascent sulphur arising from the decomposition of sulphuret of potash by an acid did not produce any effect†.

A current of cyanogen gas passed through a solution of the salt is gradually absorbed, and it becomes of a very dark colour, but red ferrocyanate is not formed.

Doubtless many may be surprised that the action of iodine has not been adverted to before, and more especially that it should not have been mentioned with chlorine and bromine, as to these it has a striking analogy in most of its properties; but in reality little resemblance exists between the action of iodine on the ferrocyanate of potassa, and that of chlorine and bromine, as we shall immediately see. If iodine is added to a solution of the salt it speedily becomes dissolved, the solution turning to a dark red, and gives a blue precipitate with salts of either oxide of iron. One equivalent of ferrocyanate of potash dissolves about one equivalent of iodine, which remains in great part uncombined in solution. If the solution is allowed spontaneously to evaporate the free iodine passes off, and a whitish uncrystallized mass is obtained which has no free iodine, but hydriodate of potassa in its composition. This gives a precipitate with both oxides of iron. Now there is a ready method of ascertaining how much iodine the ferrocyanate will not only dissolve, but combine with, and for this purpose a definite quantity of the salt is to be dissolved in a small quantity of water, and then placed in a phial. Upon the solution æther is to be poured, then the iodine is to be added gradually, when as soon as the æther is discoloured the saturation is known to be effected. Brisk and continued agitation must follow each addition of the iodine, in order that the æther may part with any iodine previously to the point of saturation. When evaporated to dryness more of the iodine is evolved, but still hydriodate of potash may be abstracted from the mass by alcohol. When all the iodine is removed

* No change takes place if the phosphorus is completely under the solution of the salt.

† It is foreign to this page to describe the sulphocyanuret of potassium.

from the mass, a result which is known by its not discolouring starch upon the addition of nitric acid, it still retains its power of forming Prussian blue with salts of either oxide of iron, and still presents the same indisposition to crystallize, for it neither shows itself as the yellow nor the red ferrocyanate of potash, but as a compound having properties intermediate with both.

When iodide of potassium is added to the ferrosesquicyanuret, iodine is evolved, the solution loses its red colour, and the salt possesses the characters similar to the mass obtained by the action of iodine on the ferrocyanate of potash. Thus it is evident that if a solution of persulphate of iron be treated with the red ferrocyanate whilst an iodide is present, Prussian blue will be formed.

Whether this is really a mixture of the ferrocyanuret and ferrosesquicyanuret or a distinct compound, it is difficult to determine, but the latter is rendered probable from its generally presenting itself as an amorphous mass; yet, however, when the purified mixture is dissolved two or three times in water, a dark mass is deposited, and at last crystals of the yellow salt are formed.

Every method which has been discovered of converting the ferrocyanate of potassa into the ferrosesquicyanuret has now been detailed, and we have seen that they may each be referred to the class of anions, for of the cations the powerful agency of potassium was unable to effect this change.

Upon the first formation of the ferrosesquicyanuret the colour will occasionally be a very dark red, but this is an adventitious, not a necessary property; for when prepared by peroxide of manganese or chloride of soda, it does not possess this dark colour. If the red crystals be carefully picked and re-dissolved, in no instance is this seen, and in every case where the dark red exists it yields to liquor ammoniæ or potassæ, with the production of a small quantity of the ferrocyanate.

The ferrosesquicyanuret, however prepared, has the same peculiar properties. It has been already mentioned that the protosalts are precipitated blue, whilst the persalts are not effected by this agent; however, the solution in the latter case is always much darkened, and after a time a small quantity of dark-coloured substance is deposited. The mode of preparation of the ferrosesquicyanuret does not influence this result.

With almost every acid, especially if heat be applied, Prussian blue is formed and hydrocyanic acid is given off, and thus upon testing for minute quantities of metal, care must be taken to prevent any excess of acid, as in that case the chemist would

find iron in everything he examines. With excess of alkali, on the contrary, no precipitate of Prussian blue is produced; and therefore if search be made for that most useful of all metals, the experiment would declare that iron had no real existence; but if the golden mean be employed, or the solution be but very slightly acid, the ferrosesquicyanuret, as well as the ferrocyanuret, become most valuable and delicate tests, the one for the peroxide, the other for the protoxide of that metal.

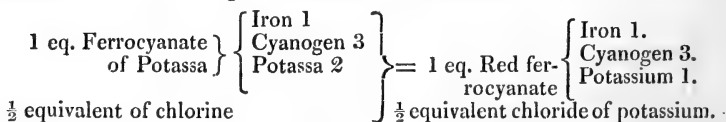
The change by chlorine and bromine has been shown to result from the abstraction of the half equivalent of potassium by the formation of chloride or bromide of that metal, and therefore the ferrosesquicyanuret is impure till that is removed by alcohol. We have seen also that the change may be effected by the iodic, nitric, and chloric acids, but by these methods the salt is also contaminated to a great extent by the nitrate of potash, but to a much less extent with the chlorate, and scarcely at all with the iodate; with phosphorus the salt in a very impure state may still be made. With peroxide of manganese, however, and the galvanic current, it may be made of absolute purity.

This last mode will probably supersede entirely every other mode of preparation, as with a galvanic battery a large quantity can be readily made. The battery which I have used for these experiments is the platinized silver, which from its simplicity is so well adapted for general purposes, and suitable for long-continued action.

Bank of England,
Feb. 12, 1840.

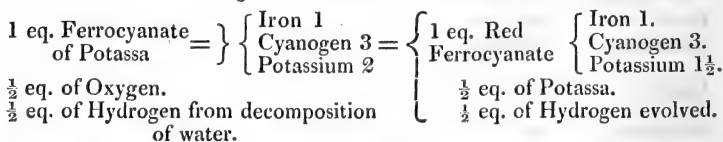
Table of Decompositions.

By Chlorine and Bromine.



Bromine acts in the same way.

By the Galvanic Current.



The action of the acids, &c. have been already sufficiently adverted to.

Table of Precipitates with the Iodo-ferrocyanate of Potassa pure.

Gold	Solution red, no precipitate.
Platinum	a little white deposit.
Mercury, bichloride	white, becoming green.
Lead	white, abundant.
Silver	white, with a little reddish tinge.
Bismuth	white, afterwards yellow.
Zinc	white.
Copper	dark brown.
Iron protosalts . . .	Prussian blue.
Iron persalts	Prussian blue.

Table of Precipitates with the Red Ferrocyanate of Potassa.

Gold . . .	chloride . . .	solution darker, no precipitate.
Platina. . .	chloride . . .	solution darker, small crystals deposited.
Palladium .	nitrate	red-brown precipitate.
Silver. . . .	{ nitrate sulphate acetate }	deep orange.
Nickel . . .	nitrate	red brown.
Copper . . .	{ sulphate ammoniuret }	yellow brown. deep-greenish brown.
Mercury . . .	{ protonitrate bichloride }	at first yellow brown, then white, none. [then green.
Bismuth . . .	nitrate	pale yellow brown.
Tin	protochloride	white, gelatinous.
Iron	{ protosulphate persulphate }	Prussian blue. none, with iodide, potassium, Prussian blue.
Antimony . .	{ potassio- tartrate }	none.
Manganese . .	chloride.	sepia.
Cobalt	chloride	chocolate brown.
Zinc	sulphate	buff.
Cadmium . . .	sulphate.	pale yellow.
Curanium . .	nitrate	deep red brown.
Lead	acetate	solution brownish, none.
Alumina . . .	acetate.	none.
Baryta	{ muriate nitrate }	none.
Strontia . . .	nitrate.	none.
Lime	muriate	none.

XXVIII. *Mineralogical Notices. Communicated by W. H. MILLER, Esq., Professor of Mineralogy in the University of Cambridge.*

[Continued from p. 105.]

ANALYSIS OF MONAZITE.

[From Poggendorff's *Annalen*, vol. xlvii. p. 385.]

ONE hundred parts of Monazite, the Mengite of Mr. Brooke, analysed by M. Carl Kersten, gave

Oxide of cerium	26.00
Oxide of lantanum	23.40
Thorina	17.95
Oxide of tin	2.10
Protoxide of manganese	1.86
Lime	1.68
Phosphoric acid	28.50
Traces of potash and titanac acid.	

ANALYSES OF OCTAHEDRAL COPPER PYRITES. BY M. PLATTNER.

[From Poggendorff's *Annalen*, vol. xlvii. p. 351.]

	From Condurrow Mine near Cam- borne, in Corn- wall.	From the Woitzki Mine near the White Sea.	From the Mär- tan mountain in Dalarne in Sweden.
Sulphur...	28.238	25.058	25.804
Copper ...	56.763	63.029	56.101
Iron	14.843	11.565	17.362
		Silica...	0.120
	From Eisleben.	From Sange- hausen.	Unknown locality. (Analysed by M. F. Varrentrapp.)
Sulphur...	22.648	22.584	26.981
Copper ...	69.726	71.002	58.199
Iron	7.539	6.406	14.845

XXIX. *On the Use of Hydriodic Salts as Photographic Agents.*
By Mr. ROBERT HUNT.*

To the Editors of the Philosophical Magazine and Journal.
GENTLEMEN,

I HAVE been engaged more than twelve months in studying the peculiarities of the salts of hydriodic acid, when used for the production of those photographic pictures which are formed by one operation, having their lights and shadows correct as in nature; and the results of my very numerous

* Communicated by the Author, whose former papers on Photography will be found in vol. xvi. p. 138, 267: see also our report of the proceedings of the Royal Society for the past session.

experiments are, the establishment of fixed principles, which remove most of the uncertainty attending the use of the hydriodates, an explanation of many of the anomalous results they give, and the discovery of some very remarkable properties not before noticed.

The uncertainty attending the application of the hydriodates, has greatly circumscribed their use, and it is the desire of forwarding the progress of a beautiful art, which now makes me solicit a few pages of your valuable Journal.

Sir John Herschel, in his very excellent memoir "On the Chemical Action of the Rays of the Solar Spectrum," &c.* particularly notices the inconstancy of the effects exhibited by the hydriodates. "Nothing," says that talented and indefatigable inquirer, "can be more variable and capricious than the results obtained according to the different intensities of the solutions applied; the qualities of the paper; the degree of darkening induced on the paper before the application of the ioduretted solution, the state of the paper as to moisture or dryness, and other circumstances."

That the various positions I wish to establish may be completely understood, and to ensure the same results in other hands, it will be necessary to enter into a somewhat detailed account of several kinds of paper which have been used, and to give tolerably full directions for successfully using the same, either in the camera obscura, or for drawings by application.

1. *The preparation of the paper.*—The variable texture of even the finest kinds of paper occasioning irregularities of imbibition, is a constant source of annoyance, deforming the drawings with dark patches, which are very difficult to remove; consequently my first endeavours were directed to the formation of a surface on which the photographic preparations might be spread with perfect uniformity.

2. A variety of sizes were tried with very variable results. Nearly all the animal glutens appear to possess a colorific property, which may render them available in many modifications of the processes published by Mr. Talbot, but they all seem to protect the darkened silver from the action of the hydriodic solutions. The gums are acted on by the nitrate of silver and browned, independent of light, which browning considerably mars the effect of the finished picture. It is a singular fact that the tragacanth and acacia gums render the drawings much less permanent. I therefore found it necessary for general practice to abandon the use of all sizes, except such as enter into the composition of the paper in the manufacture.

[* An abstract of Sir J. Herschel's paper appeared in vol. xvi. p. 331.]

3. It occurred to me that it might be possible to saturate the paper with a metallic solution, which should be of itself entirely uninfluenced by light, on which the silver coating might be spread without suffering any material chemical change. The results being curious, and illustrative of some peculiarities to be explained when the hydriodates come under our examination (65.), I shall record a few of them.

4. *Sulphate and Muriate of Iron.*—These salts, when used in certain proportions, overcame many of the first difficulties, but all the drawings on papers thus prepared faded out in the dark.

5. *Acetate and Nitrate of Lead.*—The salts of lead I have since perceived have been used by Sir John Herschel with success in some of his *negative* processes. I found a tolerably good result when I used a *saturated* solution of the above-named salts; but papers thus prepared required a stronger light than other kinds to give good results; when I used weaker solutions the drawing was covered with black patches. On these a little further explanation is required. When the strong solution has been used, the hydriodic salt which has not been expended in forming the iodide of silver, which, it is well known, is the lights of the photograph, goes to form an iodide of lead. This iodide is soluble in boiling water, and is thus easily removed from the paper. When the weaker solution of lead has been used, instead of the formation of an iodide, the hydriodate exerts one of its peculiar functions in producing an oxide of the metal (65—67.).

6. *Muriate and Nitrate of Copper.*—These salts, in any quantities, rendered the action of the hydriodates very quick, and when used in small portions appeared to promise much assistance in quickening the process; but experience has shown their inapplicability, the edges of the parts in shadow being destroyed by chemical action.

7. *Chloride of Gold.*—I did not anticipate much from the use of this salt. On trial it was found to remain inactive until the picture was formed, when a very rapid oxidation of the gold took place, and a consequent darkening of all the bright parts (5.) (65—67.).

8. Chloride of platina was found to act in all respects similarly to the chloride of gold, the re-darkening of the lights being much more rapid and intense (5. 7. 67.).

9. A very extensive variety of preparations were tried with like effects, and I was at length convinced, that the only plan by which a perfectly equal surface could be obtained, without impairing the sensitiveness of the paper, was careful manipulation with the muriated and silver solutions.

By attention to the following directions, simple in their character, but arrived at by a long series of inquiries, any one may prepare photographic papers, on which the hydriodic solutions shall act with perfect uniformity.

10. Soak the paper for a few minutes in a muriated wash, removing with a soft brush any air-bubbles which may form on it. The superfluous moisture must be wiped off with very clean cotton cloths, and the papers dried at common temperatures. When dry, the paper must be pinned out on a board, and the silver solution spread over it boldly but lightly, with a very soft sponge-brush. It is to be instantly exposed to sunshine, and, if practicable, carried into the open air; as the more speedily evaporation proceeds, the less does the silver penetrate the paper, and the more delicate it is. The first surface is very irregular, being made up of blue streaks, which are parts on which a true chloride is formed; and of brown ones, which appear to be the chloride of silver combined with a portion of undecomposed nitrate. As soon as the surface appears dry the silver solution must be again applied as before, and the exposure repeated. It must now be exposed until a fine chocolate-brown colour is produced equally on all parts of the surface, and then, until required for use, carefully preserved from the further influence of light.

11. In darkening these papers, the greatest possible attention must be paid to the quantity of light to which they are submitted, everything depending on the rapidity of the blackening process. The morning sun should be chosen, it being very evident that some portion of the violet rays are absorbed by the atmosphere after the sun has passed the meridian, which permeated it freely before he had arrived at that point.

A perfectly cloudless sky is of great advantage. The injurious consequence of a cloud obscuring the sun during the last darkening process, is the formation of a surface which has the appearance of being washed with a dirty brush. This is with difficulty removed by the hydriodates, and the resulting pictures want that clearness which constitutes their beauty. Papers darkened by the diffused light of a cloudy day are scarcely, if at all acted on by these salts.

12. The kind of paper on which the silver is spread, is an object of much importance. A paper known to the trade as satin-post, double-glazed, bearing the mark of J. Whatman, Turkey Mill, is decidedly superior to every other kind I have tried.

The demy printing papers are many of them bleached by chlorine, after an artificial substance has been given them by lime. These reverse the photographic process, and the parts

on which the light acts with the most power become the darkest of the drawing, while the shaded parts are whitened.

The dark specks which abound in some kinds of paper must be avoided, and the spots made by flies very carefully guarded against. These are of small consequence, indeed are not noticed during the darkening action; but when the hydriodic wash is applied they form centres of chemical action, and the bleaching process goes on around them independent of light, deforming the drawing with small rings, which are continually extending their diameters.

13. *The Muriated Solutions.*—These saline washes may be considerably varied, and combined to an indefinite extent with a continued change of effect, which is singularly interesting. In their application I am invariably guided by the combining proportion of the salt; for having tried solutions of all strengths, I am at length satisfied no other proportions give such certain results; consequently I always work with my scale of equivalents at hand. The following is a list of the salts I most frequently use, selected from upwards of seven hundred combinations which I have tried. They are placed in the order of sensitiveness they appear to maintain, when used under as nearly as possible the same circumstances.

Colour of Picture.

<i>a.</i> Muriate of ammonia ...	{ Red, changing to black in sunshine.
<i>b.</i> Chloride of sodium	Ditto.
<i>c.</i> Muriate of strontia	Brown, changes but slightly.
<i>d.</i> Muriate of baryta.....	{ A rich brown inclining to purple, darkens slightly.
<i>e.</i> Sol. chloruret of lime .	Very red.
<i>f.</i> Sol. chloruret of soda .	Red, changes a little.
<i>g.</i> Iodide of potassium	Yellow brown.
<i>h.</i> Chlorate of potassa	{ Variable, sometimes yellowish, often of a steel blue.
<i>i.</i> Phosphate of soda.....	Mouse colour.
<i>k.</i> Urate of soda.....	Yellow brown.
<i>l.</i> Muriate of iron	Deep brown, blackens.
<i>m.</i> Bromide of sodium	Red brown.

The change I mention in the colour of the finished picture is that which arises from a fresh exposure to the solar rays; where no change is mentioned, it is too slight to be worth notice. This phænomenon will, however, occupy our attention presently (38.). In addition to the salts named I sometimes use

Colour of Picture.

<i>n.</i> Hydrochloric acid	Red which blackens.
<i>o.</i> Hydrochloric æther ...	Black.
<i>p.</i> Aqueous chlorine	Red, deepens a little.
<i>q.</i> Phosphoric acid	Very variable.

14. When papers prepared with any of the above, except *i* and *q*, are soaked for a little time in water, and dried in the sunshine, the picture produced—it matters not what hydriodate is used—is rendered peculiarly red, and does not change by re-exposure: washing either of the papers *b*, *c*, or *d* with weak solution of ammonia, occasions this peculiarity in a striking manner.

15. *The Solution of Silver.*—Take of crystallized nitrate of silver 120 grains, distilled water 12 fluid drachms; when the salt is dissolved, add of alcohol 4 fluid drachms, which renders the solution opaque. After a few hours a minute quantity of a dark powder—oxide of silver?—is deposited, and must be separated by the filter.

16. The addition of the alcohol to the solution was adopted from an observation I made of its influence in retarding the chemical action of the hydriodates on the salt of silver, which goes on in the shade. Its use is therefore to make the action depend more on luminous influence than would be the case without it.

17. Nitric æther and acetic æther not only check the bleaching process in the shade, but actually act with the hydriodic salts in exalting the oxidation of the silver. In copying lace or feathers, they are very valuable agents, but for any other purposes they are useless, as all the faintly lighted parts are of the same tint.

18. The hydrochloric æther, which I use as the solvent of the silver, and apply without any saline wash, has a similar property to the nitric; but as it is readily affected by faint light, it is of greater value. However, papers prepared with it must be used within twenty-four hours, as after that they quickly lose their sensitiveness, and soon become nearly useless.

19. *The Hydriodic Solutions.*—To fix with any degree of certainty the strength of the solution of the hydriodic salts, which will in all cases produce the best effect, appears to me impossible; every variety of paper, either as regards its composition, or the intensity of light to which it has been exposed to darken, requiring a solution of different specific gravity.

20. *Hydriodates of Potassa and Soda.*—The former of these salts being more easily procured than any other of the

hydriodates, is the one generally employed. The strength at which I use these salts for most kinds of paper is thirty grains to an ounce of water. The following results will exhibit the different energies manifested by these solutions at several strengths, as tried on the same paper by the same light.

120	grains of salt to an ounce of water	} 12 minutes.			
	took to whiten				
100	do.	to	do.	10	—
80	do.	to	do.	9	—
60	do.	to	do.	7	—
40	do.	to	do.	6	—
30	do.	to	do.	4	—
20	do.	to	do.	6	—
10	do.	to	do.	12	—

The other hydriodic salts correspond nearly with these in their action; a certain point of dilution is necessary with all.

21. *Hydriodate of ammonia*, if used on unsized paper, has some advantage as to quickness over either the salts of potassa or soda. This preparation is, however, so readily decomposed, that the size of the paper occasions a liberation of iodine, and the consequent formation of yellow-brown spots.

22. *Hydriodate of Iron*.—This metallic hydriodate acts with avidity on the darkened paper; but even in the shade its chemical energy is too great, destroying the sharpness of outline and impairing the middle tints of the drawing. It also renders the paper very yellow.

23. *Hydriodate of lime* acts similarly to the iron, but less energetically, and the paper is not rendered yellow by it.

24. *Hydriodate of manganese* answers remarkably well when it can be procured absolutely free of iron. When the manganese solution contains it, even in the smallest quantities, light and dark spots are formed over the picture, which give it a curious speckled appearance.

25. *Hydriodic acid*, if used on paper which will not decompose its aqueous solution, acts readily on the darkened silver. It is difficult, however, to procure a paper which does not liberate the iodine. A portion of hydriodic acid, free, in any of the saline solutions, greatly quickens the action.

26. *Hydriodate of baryta* possesses advantages over every other simple hydriodic solution, both as it regards quickness of action, and the sharpness of the outline in the photograph.

27. I find, however, the quickness of this solution may be much increased. Forty grains of the hydriodate of baryta being dissolved in one ounce of distilled water, thereto should be added five grains of pure sulphate of iron, and allowed slowly to dissolve. Sulphate of baryta is precipitated, which

should be separated by filtration, when the solution is composed of the hydriodate of baryta and iron. By now adding a drop or two of very dilute sulphuric acid, more baryta is precipitated and hydriodic acid left free. The clear solution must be decanted off, as the filtering through paper decomposes the acid. By this means a photographic fluid of great value is formed. It should be prepared in small quantities, as it suffers decomposition under the influence of the atmosphere and of light. It is always easy to set hydriodic acid free by precipitating sulphate of baryta.

28. *Directions for taking Photographs.*—For drawings by application less care is required than for the camera obscura. With a very soft flat brush apply the hydriodic solution on both sides of the prepared paper until it appears equally absorbed, place it in close contact with the object to be copied, and expose to sunshine. The exposure should continue until the light parts of the picture (iodide of silver (54.)) are seen to brown. The observance of this simple rule will be found of very great advantage in practice. Immersion for a short time in soft water removes the brown hue, and renders the bright parts of the picture more clear than they would otherwise have been.

29. If the paper is intended to be used in the camera, it is best to soak it in the hydriodic solution, until a slight change is apparent from the chemical action on the silver; it is then to be stretched on a frame, and not allowed to touch in any part but at the edges; placed in the dark chamber of the camera at the proper focus, and submitted to luminous influence.

If the wetted paper is placed upon any porous body, it will be found, owing to the capillary communication established between different points, that the solution is removed from some parts to others, and different states of sensitiveness induced. Another advantage of the frame is, the paper being by the moisture rendered semi-transparent, the light penetrates and acts to a greater depth, thus cutting out fine lines which would otherwise be lost. However, if the camera is large, there is an objection to the frame; the solution is apt to gather into drops, and act intensely on small spots to the injury of the general effect. When using a large sheet, the safest course is to spread it out when wetted upon a piece of very clean wet glass, great care being taken that the paper and glass are in every part in close contact. The picture is not formed so quickly when the glass is used as when the paper is extended on a frame, owing to the evaporation being slightly retarded; the additional time required, about one-

sixth longer, is however in most cases of small consequence. It is somewhat singular that if the glass plate is interposed between the paper and the lens, the action is not more retarded than if it had been placed behind it. The interference of a transparent plate is little felt in the hydriodic process.

30. *On fixing these Photographs.*—The picture being formed by the influence of light, it is required, to render it unchangeable by any further action of the luminous fluid, not only that the hydriodic salt be entirely removed from the paper, but that the iodide of silver which is formed be also dissolved out of the drawing.

31. By well washing the drawing in warm water the hydriodate is removed, and the pictures thus prepared have been stated to be permanent; and if they are kept in a portfolio, and only occasionally exposed, they are really so; for I shall show presently (54.) that they have the property of *being restored in the dark to the state in which they were prior to the destructive action of light.* I have now before me the first drawing of this kind I ever executed, bearing the date June 17, 1839. This drawing has been kept loosely in my table drawer, and has often been exposed for many successive days to the action of the sun; yet the most delicate venations of the rose leaves are as perfect as at first. Thus prepared, however, these photographs will not bear continued exposure without injury, about three months in summer, or six weeks in winter being sufficient to destroy them.

32. For a long period I was under the impression that two iodides of silver existed, the one sensitive to solar influence, but the other not so; and in my paper published in your Magazine for April, I stated such to be my opinion. I have, however, since that period seen reason sufficient to question the correctness of my conclusion. Under the former impression, not being successful in removing the iodide from the paper without also injuring the oxidized or dark portions, I endeavoured to effect a chemical change in the iodide of silver. Some of the results being curious, I shall give them.

33. By washing the photograph with a hot saturated solution of the acetate of lead, the yellowness of the lights was at first increased, but eventually considerably whitened, and the dark parts assumed a peculiar crimson hue. The drawing faded out entirely by the action of light in three weeks.

34. When these drawings are dipped into a solution of the bichloride of mercury, they fade out in precisely the same manner as Sir John Herschel discovered the photographs on Mr. Talbot's principle were obliterated, and in like manner

are they restored by a liquid hyposulphite; the paper, instead of being completely white, being altogether of a full rich yellow. When these photographs are restored by the hyposulphite, they are even less permanent under the influence of light than those washed with the salt of lead.

35. The ferrocyanate of potassa exerts no action on these photographs in any way remarkable, unless they have been formed by the agency of the hydriodate of iron (22.) or of baryta and iron (27.). They are then obliterated by it, but on exposure, the light parts of the picture are darkened, changing thus to a *negative* photograph, the originally dark parts being now a light blue.

36. With much attention, I have tried the hyposulphites of soda, ammonia and potassa. But I have failed to remove all the iodide of silver, without destroying at the same time the dark parts, and the minute portion which remains in the paper is very soon darkened by light to a tint similar to the lighter shades of Indian ink. When first done the drawing is much improved in appearance, but it is difficult to remove the hyposulphite so completely as is necessary to prevent the formation of the sulphuret of silver.

37. Sulphuretted hydrogen gas, which has the singular property of blackening the iodide of silver, when in that state which is easily darkened by light, *but of bleaching it in the less susceptible state*, acts on these photographs in a manner similar to the hyposulphites; but the oxidized portions of the picture are first destroyed and then restored by light. The light parts are, however, rendered brown.

I have tried a great variety of other agents, diversifying my method of using them in almost every possible way, but as yet I have discovered no material which effectually removes the iodide of silver alone; consequently I satisfy myself with well washing my photographs in hot water.

[To be continued.]

XXX. On the *Anthracite Coal of South Wales.* By SAMUEL WOODS, Esq., F.G.S.

To the Editors of the *Philosophical Magazine and Journal.*

GENTLEMEN,

HAVING received from a friend at Paris the accompanying analysis of some of the anthracite coal found abundantly in this neighbourhood, I transmit it to you in the belief that it may be worthy of record in your pages; and the quality and uses of this coal having lately excited so much

attention and inquiry, a few preliminary observations may not prove unacceptable.

This deposit of anthracite coal forms a part of the large Welsh coal basin extending from Brides-bay to Pontypool, —the western limit of the anthracite (or stone coal) deposit being the coast of Pembrokeshire, and its eastern terminating not far beyond the Neath Valley: the seams towards Merthyr and beyond, gradually losing their anthracetous and assuming the bituminous character, the width till it reaches Llandshipping in Milford-haven is inconsiderable, from whence it slowly expands towards Tenby. In this district there do not appear to exist more than three or four distinct veins, all of which seem to have been exposed to intense heat, and to disturbing forces of great violence, these veins abounding in faults and tossed about in various directions, very difficult to follow, a large proportion being reduced into small particles known by the denomination of culm, employed chiefly in lime-burning: the larger pieces, however, possess a very uniform compact character and superior purity, and are in great reputation and demand for drying malt, for which purpose they have been long used. The veins then appear to pass under Carmarthen-bay, and again to emerge in the Gwendraeth Valley beyond Kidwelly, where a vast quantity is procurable. From the southern edge of the basin from Pembrey to Swansea and Neath, the coal is chiefly bituminous, but occasionally of a mixed character, more or less approaching to anthracite, the latter becoming more pure and distinct as we advance northward through the collieries of Trimsaran, Brondyny, Llangennyh and Pont-twrch in the Swansea Valley. To the north of all these points the quality rapidly improves, and all the coal north of a line drawn from Pont Yales to Aperpergwm, may be considered real anthracite. The Gwendraeth Valley from near Pont Yales to the Big mountain, a distance of about eight miles trending N.E. is known to possess thirteen distinct veins, somewhat varying in purity and thickness, the latter in the aggregate amounting to about 40 feet, and the greatest depth hitherto worked, or in contemplation to work, about 65 fathoms; the dip from 30° to 45°. Seams of argillaceous iron ore are also found between the coal veins.

The use of this coal has hitherto been chiefly confined to maltsters; it has been more recently adopted for Dr. Arnott's and other stoves, and is now successfully applied to the reduction of iron ores in the vicinity of Swansea; and its freedom from smoke, joined to the durability of its heat, offer the strongest recommendations for its employment in locomotive engines either on land or water, and there is little doubt will

be ultimately adopted for these purposes. There seem at present some difficulties as to the best mode of using this combustible; since by sudden exposure to an incandescent furnace it is apt to split or exfoliate into minute fragments, which choke up the draught of air, or are thrown out like dust by any rapid motion. Dr. Ure assigns as the cause of this, I believe truly, that being a bad conductor of heat, the superficial parts expand and break off from the cooler internal portions; the remedy for which appears to be some contrivance for gradually heating the coal before it comes into contact with the fire, and of supplying a constant current of hot air; these precautions are not required for common fires. Mr. Player has secured a patent for such a process, which is exhibited on the Thames in the steamer called the Anthracite, the action of which every one is disposed to praise, yet no one adopts: for this no reason appears but the difficulty of overcoming prejudices, or the fear of engaging in novelties in the first instance requiring some additional expenditure.

With regard to its application to domestic use it has many and forcible recommendations; it gives out a clear, steady and durable heat; requires but little attention when once lighted; and the absence of all annoyance from smoke, soot or dust, renders it very desirable for culinary purposes and for bedrooms; in the parlour it may be thought deficient in the bright and cheerful character which belongs to the Newcastle coal; besides which, as the anthracite never cakes, it requires no aid from the poker, the employment of which on the contrary extinguishes the fire, and therefore may be deemed objectionable.

Analysis of the vein of Anthracite coal called the Gwerdd (Green) vein from Coalbrook in Carmarthenshire near Pont y berem, in the vale of the Gwendraeth, the property of the Gwendraeth Anthracite Company; by Mons. Jacquelin, of the *Ecole des Arts* at Paris.

Ultimate analysis.		Manufacturing analysis.	
Carbon ...	89.43	Carbon 89.80	} Coke 91.50
Hydrogen	3.56	Ashes . 1.70	
Oxygen ...	3.66	Water 1.35	
Azote.....	0.29	Volatile sub- stances { slightly car- buretted hydrogen, ammonia and oxygen } 7.15	}
Ashes	1.70		
Hygrome- tric moist- ure.	1.36		
<hr/> 100		<hr/> 100	

One gramme of the coal reduces 33.3 grammes of lead, consequently the heating power may be thus estimated: 1 kilo-

gramme is capable of raising 76·54 kilogrammes of water (quere from 32°) to the boiling point, or of evaporating 11·55 kilogrammes. All the samples of coal placed in my hands were of a brilliant black, very compact, and of a lamellar structure; the cross fracture rough and uneven; they do not soil the fingers, and break easily under the hammer; the hardness is nevertheless considerable, and it is difficult so to reduce it to powder as to destroy its brilliancy.

The double carbonate of lime and iron occurs between the laminæ of this coal, and sometimes agglomerations of carbonaceous matter possessing the appearance, lightness and friability of wood charcoal.

The specific gravity of this coal is 1·27; it burns in the furnace without flame; a small quantity reduced to powder consumes slowly without inflaming; ignited masses do not lose their form, but when separately exposed to the air become extinguished, whereas in mass the combustion succeeds well in the reverberatory furnace.

I incinerated some of this coal reduced to powder in a muffle furnace charged with the same coal, and beginning with a stratum of lighted charcoal: the combustion continued for five hours, while every part of the interior of the furnace was incandescent.

The residue of this incineration upon a small scale, consists of slightly ferruginous and calcareous ashes; that of combustion on the large scale is similar, but mixed with some small pieces of coal having suffered incipient exfoliation. The ready combustion in mass is undoubtedly to be attributed to a slight separation of the laminæ of the coal produced by the high temperature, and to the emission of a gas*.

The quantity of gas which this coal is capable of supplying in close vessels has been found to be 24 litres for 100 grammes of coal, equivalent to 240 litres for a kilogramme. It will be seen that these results approach those obtained on the large scale in gas-works, where that quantity yields 180, 200, and 250 litres of gas; unfortunately the gas produced from this anthracite does not give more light than pure hydrogen; it is now well known that illuminating power may be communicated to hydrogen gas by causing it to circulate in reservoirs over the surface of oil of schist or of tar.

From what has been stated, it is evident that coal having a uniform character similar to that which I have analysed, would be much in demand on account of its remarkable purity, both for domestic consumption and for blast furnaces,

* I do not exactly comprehend how this separation of the laminæ promotes combustion. I should have thought its tendency would have been to choke and check the fire.—S. W.

and especially for the latter, on account of the extremely high temperature it is capable of producing.

I repeat, that if this combustible prove homogeneous and of equal quality in every part of the deposit, the discovery is one of great importance for the reduction of ores and the quality of the resulting products.

Dr. Schafhaeutl of Munich, now or lately residing at Swansea, has analysed for the Kilgetty Company, two samples of the Pembrokehire coal; the average specific gravity he states to be 1·413.

1.	2.
Carbon... ..	92·42
Hydrogen.....	3·37
Oxygen.....	1·43
Nitrogen.....	1·05
Sulphur.....	·12
Earthy matter	1·61
Alumina.....	·478
Silica	·190
Iron	·264
Water	·300
	100

Pembrey, Carmarthenshire.

XXXI. *On the Tension Spark from the Voltaic Battery.* By ANDREW CROSSE, Esq. Communicated in a Letter to John P. Gassiot, F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE inclosed communication from my friend Andrew Crosse, Esq., may perhaps be interesting to the readers of the Philosophical Magazine; the paper he alludes to is one I communicated to the Royal Society, and which was honoured by a place in their Transactions of this year.

I am, Gentlemen, yours, &c.

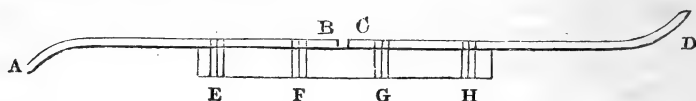
Clapham Common, Aug. 3, 1840.

JOHN P. GASSIOT.

MY DEAR SIR,

I return you my best thanks for your paper on the voltaic spark, &c.; I have read it with attention, and was much interested by its perusal, particularly by that part which relates to Zamboni's pile. I once had a cork ball kept vibrating between the poles of four columns of De Luc's pile, without cessation, for upwards of twelve years! Now for the possibi-

lity of obtaining a spark between the poles of a voltaic battery before the circuit is completed. Were you to see the action of my unfinished water battery of 1626 pairs of zinc and copper cylinders, you would allow the question to be set at rest. I take a small glass stick, and tie on it, with waxed silk thread, very securely, two wires of platina, with the two extreme ends ready to be plunged into two cups of mercury connected with the opposite poles of the battery: the two other ends of the wires are brought to the distance of about $\frac{1}{100}$ th of an inch from each other, as below.



A B, C D, two platina wires secured on the glass stick E, F, G, H at the parts E, F, G, H. The two nearest ends of the wires approach each other at B, C to about the distance of $\frac{1}{100}$ th of an inch. I say about, for I have no instrument to measure it with accuracy, nor is it of any consequence, as the cells of that battery are not by any means so well insulated, as that the above distance should be taken as a test of intensity of the battery. The moment the connexion is made with the poles of the battery, a small stream of fire takes place at the interval between B and C, which I have kept up for many minutes, nor did it appear inclined to cease. This experiment never fails, but with a much greater number of plates, each pair not being separately insulated, it would never succeed. To expect to produce a spark or visible current under similar circumstances with the above would be hopeless, except with a considerable number of pairs of plates, each pair being separately insulated. With 1200 pairs I have succeeded, and with 10,000 or 20,000 the distance at which it would strike would be very great, comparatively speaking.

I showed a friend the other day about twelve inches of iron chain illuminated pretty strongly by the passage of repeated shocks of my large electrical battery through it, charged by the water battery alone. The intensity was so great as to keep up a constant dance of pieces of silver-leaf between two plates connected with the opposite poles. The reason why Professor Daniell's water battery, which you employed in some of your experiments, failed, was first, that the cells were not separately insulated, and secondly, that they were too few in number. I presume it was the water battery which I once saw used at the Royal Institution, the intensity of which was very feeble.

I do wish you could manage to pay me a visit, and see the

action of my battery. I mean, as soon as I have time to add about 850 pairs of cylinders to it. Woe then to the unfortunate wretch who comes between the poles, when connected with the electrical battery!

P. S. Please to observe, that when I procured the stream of electricity in the interval between the platina wires, I used the water battery alone, without other apparatus, and not connected with the electrical or any other battery.

Broomfield, near Taunton, July 17, 1840.

XXXII. *On the Process of Daguerreotype, and its application to taking Portraits from the Life.* By JOHN WILLIAM DRAPER, M.D., Prof. Chemistry in the University of New York.

VERY soon after M. Daguerre's remarkable process for Photogenic Drawing was known in America, I made attempts to accomplish its application to the execution of portraits from the life. M. Arago had already stated, in his address to the Chamber of Deputies, that M. Daguerre expected, by a slight advance, to meet with success, but as yet no account has reached us of that object being attained.

More than one hundred instances are recorded in Berzelius's chemistry, in which the agency of light brings about changes in bodies; these are of all kinds: formations of new compounds, re-arrangements of elements already in union, changes of crystallographic character, decompositions, and mechanical modifications.

The process of the Daguerreotype is to expose a surface of pure silver to the action of the vapour of iodine, so as to give rise to a peculiar iodide of silver, which under certain circumstances is exceedingly sensitive to light. The different operations of polishing, washing with nitric acid, exposure to heat, &c., are only to offer a pure silver surface; the operation of hyposulphite of soda, and the process, which I shall presently describe, of galvanization, are to free the plate from its sensitive coating, and in no wise affect the depth of the shadows, as some of the French chemists at first supposed.

There is but one part of the Daguerreotype which does not yield to theory: on one point alone there is obscurity. Why does the vapour of mercury condense in a white form on those portions of the film of iodide, which have been exposed to the influence of light?—condense to an amount which is rigidly proportional to the quantity of incident light?

Even on this point there are facts which appear to have a bearing.

(a.) It has long been known, that if a piece of soapstone or

agalmatolite be made use of as a pencil to write with on glass, though the letters that may have been formed are invisible, and though the surface of the glass may subsequently have been well cleared, yet they will come into view as soon as the glass is breathed on.

(b.) I have often noticed, that if a piece of very clear and cool glass, or what is better, a cold polished metallic reflector, has a little object, such as a piece of metal, laid upon it, and the surface be breathed over once, the object being then carefully removed, as often as you breathe again on the surface, a spectral image of it may be seen, and this singular phenomenon may be exhibited for many days after the first trial was made.

(c.) Again, in the common experiment of engraving on glass by hydrofluoric acid, if the vapour has been very weak, no traces will be perceived on the glass after the wax has been removed; but on breathing over it, the moisture condenses in such a way, as to bring all the object into view.

(d.) In a former number of this Journal I described a phenomenon which relates to the crystallization of camphor on surfaces of dry glass, on which moveable traces have been made by the pressure of a glass rod; this also appears to belong to the same class of effects.

Berzelius (*Traité*, vol. ii. p. 186.) has attempted to explain (a.) and (c.) on this principle, that the changed and unchanged surfaces radiate heat unequally. There may be strong doubts with some as to the correctness of this, but is not the Daguerreotype due to the same cause, whatever it may be?

We must separate carefully the chemical changes which iodide of silver undergoes in the sunbeam, from the mechanical changes which happen to the sensitive film: iodide of silver turns black in the solar ray, the whole success of the Daguerreotype artist depends on his checking the process before that change shall have supervened.

The coating of iodine is not *immediately* necessary to the production of images by the mercurial vapour. The condition seems to be traceable to the metallic surface. If you take a Daguerreotype, clean off the mercury, polish the plate thoroughly with rottenstone, wash it with nitric acid and bring it to a brilliant surface, yet if it has not been exposed to heat, the original picture will re-appear on exposure to the mercurial vapour. Is not this a result of the same kind as those just referred to?

As a polishing material for the Daguerreotype plate, common rottenstone and oil answer very well. The plate having been planished by the workman, is to be rubbed down to a good surface, and as high a polish given to it as possible;

it is to be heated and washed with nitric acid, as indicated in the French account, and finished by being rubbed with whiting (*creta præparata*), in the state of a very dry powder, going over it for the last time with a piece of clean dry cotton; this gives an intensely black lustre, which cannot be obtained by rottenstone alone, and thoroughly removes any film which nitric acid may have left.

To coat with iodine, I make use of a box about two inches deep, in the bottom of which that substance in coarse flakes is deposited; no cloth intervenes, but the silvered plate, with a temporary handle attached to it, is brought within half an inch of the crystals, and it becomes perfectly coated in the course of from one to three minutes; no metallic strips are necessary to ensure this effect; if the edges and corners are thoroughly clean, the golden hue will appear uniformly.

M. Daguerre recommends, that the plate, after being iodized, shall be placed in the camera without loss of time. The longest interval, he says, ought not to exceed an hour. "Beyond this space the action of the iodine and silver no longer possesses the requisite photogenic properties."

There may be something peculiar in the preparation of the plate as I have described it, but it is certain that this observation must be received with some limitation. A plate, which has been iodized, does not appear so quickly to lose its sensitiveness. On the other hand, by keeping it in the dark for twelve or twenty-four hours, its sensitiveness is *often remarkably increased*. Other advantages also accrue. Those who have made many of these photogenic experiments, will have had frequent occasion to remark, that the film of iodine is not equally sensitive all over, that there are spots or cloudy places which do not evolve any impression, and often the whole is in that condition, that the bright parts alone come out, while the parts that are in shadow do not evolve correspondingly, nor can they be well developed, except at the risk of solarizing the picture. Now, a plate that has been kept for several hours, is by no means so liable to these effects: I do not pretend to give any reason for this, but merely mention it as a fact, of considerable importance to the travelling daguerre-typer; he will find that the iodine does not lose its sensitiveness in many days.

In a paper read before the Royal Society, of which an abstract is given in the April number of this Journal for the present year (p. 333.), Sir John Herschel states, that there is an absolute necessity of a perfect achromaticity in the object-glass of a photographic camera. M. Daguerre appears to have been under the same impression, and recommends in his published account such an object-glass.

All the rays of light, with *perhaps* the exception of the yellow, leave an impression on the iodide of silver. The less refrangible rays, however, act much more slowly than those which are at the opposite end of the spectrum. In the common kinds of glass, the most energetic action takes place in the indigo, or on the boundary of the blue. Now the retina receives an impression with equal facility from each of the different rays, the yellow light acting as quickly upon it as the red or the blue. Vision is therefore performed independently of time, the eye catching all the colours of the spectrum with equal facility and with equal speed. But it is not so with these photogenic preparations. In the action of light upon them, time enters as an element; the blue ray may have effected its full change, whilst the red is yet only beginning slowly to act; and the red may have completed its change before the yellow has made any sensible impression. On these principles, it is plain that an achromatic object-glass is by no means essential for the production of fine photographs; for if the plate be withdrawn at a certain period, when the rays that have a maximum energy have just completed their action, those that are more dispersed but of slower effect, will not have had time to leave any stain. We work, in fact, with a temporary monochromatic light.

Upon these principles I constructed the camera which I am in the habit of using, with a double convex non-achromatic lens. Some of the finest proofs were procured with a common spectacle lens, of fourteen inches focus, arranged at the end of a cigar-box as a camera; a lens of this diameter answers very well for plates four inches by three, reproducing the objects with the most admirable finish, copper-plate engravings being represented in the minutest particulars, and the marks of the tool becoming quite distinct under the magnifier.

In this instance, it is true, owing to the magnitude of the focal length compared with the aperture, but little difficulty ensues from chromatic aberration; but when with the same focal length the aperture is increased to three or four inches, then the dispersion becomes very sensible, and yet good proofs can be procured, by working in the method here indicated, the chief difficulty then arising from spherical aberration.

It has already been stated, that the ray of maximum action for the Daguerreotype, when colourless French plate-glass is used, lies probably within the indigo space: it therefore follows, that the length of the camera should be diminished, after arranging it to the luminous focus. The importance of this is pointed out in a paper by Mr. Towson, inserted in this Journal last year; I was, however, in the habit of using this ad-

justment before reading the suggestions contained in that excellent communication. The amount of shortening which should be given to the camera, where the lens is fifteen inches focus, does not commonly exceed three-tenths of an inch. If the luminous focus be used, the proof comes out indistinct.

In the subsequent process of mercurializing, it is of little importance what is the angular position. Several experimenters were for a time under the idea that an angle of 45° or 48° was a necessary inclination, in order that the plate should take the vapour; this arose from a misinterpretation of the printed account. Plates mercurialize equally well in a horizontal as in any other position; perhaps a slight inclination may be of advantage, in allowing the vapour to flow with uniformity over the iodized surface, but the chief use of an angle of 45° , is to allow the operator to inspect the process through the glass.

Sometimes it is advantageous to heat the mercury a second time, when the proof is not distinctly evolved at first. Indeed, it occasionally happens, that a proof which did not evolve at all at first, will come out quite fairly on raising the temperature of the mercury again.

M. Daguerre recommends two methods of removing the sensitive coating from the plate, by washes of hyposulphite of soda, and a solution of common salt. The former answers perfectly, the second only indifferently well. There is, however, another process, which is very simple, and has an advantage over the former of these in cheapness. It adds not a little to the magic of the whole operation, in the eyes of those who are unaccustomed to chemical results. The plate, having been dipped into cold water, is placed in a solution of common salt, of moderate strength; it lies without being acted upon at all; but if it be now touched on one corner with a piece of zinc, which has been scraped bright, the yellow coat of iodide moves off like a wave and disappears. It is a very pretty process. The zinc and silver forming together a voltaic couple, with the salt water intervening, oxidation of the zinc takes place, and the silver surface commences to evolve hydrogen gas; whilst this is in a nascent condition it decomposes the film of iodide of silver, giving rise to the production of hydriodic acid, which is very soluble in water, and hence instantly removed.

This process, therefore, differs from that with hyposulphite. The latter acts by dissolving the iodide of silver, the former by decomposing it. It is necessary not to leave the zinc in contact too long, or it deposits stains, and in large plates the contact should be made at the four corners successively, to avoid this accident,

After the proof is washed, all the defects in the preparation of the plate become apparent. If a film of mercury has existed on it, due to its not having been burnt sufficiently long, there will be found a want of distinctness in the shadows; or if the plate has not been burnt at all, perhaps the former impressions which have been obtained will re-appear. This accident frequently happened in my earlier trials, when care had not been taken to give a due exposure each time to the spirit flame. Spectral appearances of former objects, on different parts of it, emerged,—an interior with Paul Pry coming out, when the camera had been pointed at a church.

There is no difficulty in procuring impressions of the moon by the Daguerreotype, beyond that which arises from her motion. By the aid of a lens and a heliostat, I caused the moonbeams to converge on a plate, the lens being three inches in diameter. In half an hour a very strong impression was obtained. With another arrangement of lenses I obtained a stain nearly an inch in diameter, and of the general figure of the moon, in which the places of the dark spots might be indistinctly traced.

An iodized plate, being exposed for fifteen seconds only close to the flame of a gas light, was very distinctly stained; in one minute there was a very strong impression.

On receiving the image of a gas light, which was eight feet distant, in the camera, for half an hour, a good representation was obtained.

The flame of a gas lamp was arranged within a magic lantern, and a portion of the image of a grotesque on one of the slides received on a plate; a very good representation was procured.

With Drummond's light, and the rays from a lime-pea in the oxy-hydrogen blowpipe, the same results were obtained.

In the first experiments which I made for obtaining portraits from the life, the face of the sitter was dusted with a white powder, under an idea that otherwise no impression could be obtained. A very few trials showed the error of this; for even when the sun was only dimly shining, there was no difficulty in delineating the features.

When the sun, the sitter, and the camera are situated in the same vertical plane, if a double convex non-achromatic lens of four inches diameter and fourteen inches focus be employed, perfect miniatures can be procured, *in the open air*, in a period varying with the character of the light, from 20 to 90 seconds. The dress also is admirably given, even if it should be black; the slight differences of illumination are

sufficient to characterize it, as well as to show each button, button-hole, and every fold.

Partly owing to the intensity of such light, which cannot be endured without a distortion of the features, but chiefly owing to the circumstance that the rays descend at too great an angle, such pictures have the disadvantage of not exhibiting the eyes with distinctness, the shadow from the eyebrows and forehead encroaching on them.

To procure fine proofs, the best position is to have the line joining the head of the sitter and the camera so arranged as to make an angle with the incident rays of less than ten degrees, so that all the space beneath the eyebrows shall be illuminated, and a slight shadow cast from the nose. This involves obviously the use of reflecting mirrors to direct the ray. A single mirror would answer, and would economise time, but in practice it is often convenient to employ two; one placed, with a suitable mechanism, to direct the rays in vertical lines; and the second above it, to direct them in an inviolable course towards the sitter.

On a bright day, and with a sensitive plate, portraits can be obtained in the course of five or seven minutes, in the diffused daylight. The advantages, however, which might be supposed to accrue from the features being more composed, and of a more natural aspect, are more than counterbalanced by the difficulty of retaining them so long in one constant mode of expression.

But in the reflected sunshine, the eye cannot support the effulgence of the rays. It is therefore absolutely necessary to pass them through some blue medium, which shall abstract from them their heat, and take away their offensive brilliancy. I have used for this purpose blue glass, and also ammoniacosulphate of copper, contained in a large trough of plate glass, the interstice being about an inch thick, and the fluid diluted to such a point, as to permit the eye to bear the light, and yet to intercept no more than was necessary. It is not requisite, when coloured glass is employed, to make use of a large surface; for if the camera operation be carried on until the proof *almost* solarizes, no traces can be seen in the portrait of its edges and boundaries; but if the process is stopped at an earlier interval, there will commonly be found a stain, corresponding to the figure of the glass.

The camera I have used, though much better ones might be constructed, has for its objective two double convex lenses, the united focus of which for parallel rays is only eight inches; they are four inches in diameter in the clear, and are mounted in a barrel, in front of which the aperture is narrowed down to $3\frac{1}{2}$ inches, after the manner of Daguerre's.

The chair in which the sitter is placed, has a staff at its back, terminating in an iron ring, that supports the head, so arranged as to have motion in directions to suit any stature and any attitude. By simply resting the back or side of the head against this ring, it may be kept sufficiently still to allow the minutest marks on the face to be copied. The hands should never rest upon the chest, for the motion of respiration disturbs them so much, as to bring them out of a thick and clumsy appearance, destroying also the representation of the veins on the back, which, if they are held motionless, are copied with surprising beauty.

It has already been stated, that certain pictorial advantages attend an arrangement in which the light is thrown upon the face at a small angle. This also allows us to get rid entirely of the shadow from the back-ground, or to compose it more gracefully in the picture; for this, it is well that the chair should be brought forward from the back-ground, from three to six feet.

Those who undertake Daguerreotype portraiture, will of course arrange the back-grounds of their pictures according to their own tastes. When one that is quite uniform is desired, a blanket, or a cloth of a drab colour, properly suspended, will be found to answer very well. Attention must be paid to the tint,—white, reflecting too much light, would solarize upon the proof before the face had had time to come out, and owing to its reflecting *all* the different rays, a blur or irradiation would appear on all edges, due to chromatic aberration. It will be readily understood, that if it be desired to introduce a vase, an urn, or other ornament, it must not be arranged against the back-ground, but brought forward until it appears perfectly distinct on the obscured glass of the camera.

Different parts of the dress, for the same reason, require intervals, differing considerably, to be fairly copied; the white parts of a costume passing on to solarization before the yellow or black parts have made any decisive representation. We have therefore to make use of temporary expedients. A person dressed in a black coat, and open waistcoat of the same colour, must put on a temporary front of a drab or flesh colour, or by the time that his face and the fine shadows of his woollen clothing are evolved, his shirt will be solarized, and be blue, or even black, with a white halo around it. Where, however, the white parts of the dress do not expose much surface, or expose it obliquely, these precautions are not essential; the white shirt collar will scarcely solarize until the face is passing into the same condition.

Precautions of the same kind are necessary in ladies, dresses, which should not be selected of tints contrasting strongly.

It will now be readily understood, that the whole art of taking Daguerreotype miniatures, consists in directing an almost horizontal beam of light, through a blue coloured medium, upon the face of the sitter, who is retained in an unconstrained posture, by an appropriate but simple mechanism, at such a distance from the back-ground, or so arranged with respect to the camera, that his shadow shall not be copied as a part of his body; the aperture of the camera should be three and a half or four inches at least, indeed the larger the better, if the object be applanatic.

If two mirrors be made use of, the time actually occupied by the camera operation varies from forty seconds to two minutes, according to the intensity of the light. If only one mirror is employed, the time is about one-fourth shorter. In the direct sunshine, and out in the open air, the time varies from under half a minute.

Looking-glasses, which are used to direct the solar rays, after a short time undergo a serious deterioration; the foil assuming a dull granular aspect, and losing its black brilliancy. Hence the time, in copying, becomes gradually prolonged.

The arrangement of the camera, above-indicated, gives reversed pictures, the right and left sides changing places. Mr. Woolcott, an ingenious mechanician of this city, has taken out a patent for the use of an elliptical mirror for portraiture; it is about seven inches in aperture, and allows him to work conveniently with plates two inches square. The concave mirror possesses this capital advantage over the convex lens, *that the proof is given in its right position, that is to say, not reversed*; but it has the serious inconveniences of limiting the size of the plate, and representing parts that are at all distant from the centre, in a very confused manner. With the lens, plates might be worked a foot square, or even larger.

Miniatures procured in the manner here laid down, are in most cases striking likenesses, though not in all. They give of course all the individual peculiarities, a mole, a freckle, a wart. Owing to the circumstance, that yellow and yellowish browns are long before they impress the substance of the Daguerreotype, persons whose faces are freckled all over give rise to the most ludicrous results, a white, mottled with just as many black dots as the sitter had yellow ones. The eye appears beautifully; the iris with sharpness, and the white dot of light upon it, with such strength and so much of reality and life, as to surprise those who have never before seen it. Many are persuaded, that the pencil of the painter has been secretly employed to give this finishing touch.

XXXIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 153.]

Dec. 18, 1839. **A** PAPER was afterwards read, entitled, "Observations on the locality of the Hyracotherium," by William Richardson, Esq., F.G.S.

In 1829, when Mr. Richardson first examined the coast from Whitstable to Herne Bay, it presented an uniform, geological structure, composed of a capping of vegetable mould, under which was a stratum 3 or 4 feet thick of yellow brick earth, containing in the upper part rolled and angular flints, mammalian remains and fossils derived from secondary strata; and beneath, forming the mass of the cliff, was London clay of a dark brown colour, abounding in septaria, selenite, pyritous wood, teeth and vertebræ of fishes, Nautili with other characteristic marine testacea, Encrinital and Penta-crinital remains, and crustaceans.

The whole of the line of coast undergoes rapid degradation in consequence of the encroachment of the sea and land springs; and the changes thus annually produced, effect great alterations in the physical outline of the cliffs. The geological structure, however, presented by them in 1829 remained for the greater part the same in the autumn of 1839, except at the part called Studd Hill. At this point, the dark brown incoherent clay had been removed, and a deep blue, tenacious one exposed. A change had also taken place in the character of the fossils, the marine remains having gradually become less prominent and been replaced by others of a fluvio-marine character. In the autumn of last year, Mr. Richardson could not find a single marine shell, and only a few fragments of crinoidal stems. Terrestrial vegetables have, however, become so prodigiously abundant, that he has obtained at different times above 500 fossil cones, fruits, and other seed-vessels; and fragments of small trees converted into pyrites occur in so great quantities, that they have been removed by barge loads for æconomical purposes, and become a source of considerable profit to the neighbouring peasantry. These remains present no indications of having been transported from a distance. Neither land nor fresh-water shells have been observed.

From the abundance of vegetables, and the knowledge that Nature ever directs her means as well in number as in fitness to particular ends, Mr. Richardson inferred, that remains either of quadrupeds or birds would be found in Studd Hill; and though his search was long fruitless, it was at last rewarded by the discovery of the portion of the Hyracotherium described by Mr. Owen in the preceding memoir.

January 8, 1840. A paper was first read, on the carboniferous and transition rocks of Bohemia, by David T. Ansted, Esq., F.G.S.

After alluding to the difficulties which beset the researches of a geologist in a country so little frequented as that visited by himself, and noticing the granite and gneiss mountains which constitute the

south-eastern and south-western boundaries of Bohemia, he proceeds to the main object of the memoir. The district described by Mr. Ansted is included within a triangle, having the country between Luditz and Pilsen for a base, and Prague for its apex; and its structure is explained by a series of sections from Luditz to Pilsen—Radnitz to Rakonitz—Zebrak to Ginetz—and Przilep to Karlstein, all of them being more or less in the dip of the strata. The formations composing the district, are granite, gneiss, graywacke, coal measures, trap rocks and accumulations of superficial debris. It is stated that a line drawn from Eger on the west to Prague on the east would completely separate the sedimentary deposits of a newer date than the carboniferous system from the coal measures and transition rocks; and that the latter occur only to the south of the line. Near Eger is a small local deposit of upper tertiary sandstone, mentioned by Mr. Ansted on account of its containing myriads of fossil infusoria cases.

Section 1. Luditz to Pilsen.—Luditz stands upon a range of round topped gneiss hills, but in a depression between two of them; and about 3 miles from the town, is a bed of thinly laminated micaceous sandstone, containing a few obscure vegetable markings, and believed by Mr. Ansted to be a recent deposit. Proceeding in the direction of Pilsen, the gneiss is succeeded by a hard cherty stone, considered by the author to belong to a rock which underlies the coal measures in other parts of the country, but to have been protruded at this point by igneous agency. The next hill is formed of trap, and beyond it, is a bed of similar cherty sandstone, covered up towards the S.E. by the red conglomerate on which Manotin is built. To the south of this town, slate rocks are finely developed for several miles, forming precipitous cliffs, with the strata dipping to the S.E. They are covered in part by gravel, and are succeeded by rotten shales, assigned by Mr. Ansted to the graywacke system. These shales are visible for only a short distance, being superficially replaced by a thick covering of gravel, which extends for ten miles. At the end of that distance, hills of sandstone commence, and contain near Pilsen workable seams of coal. The sandstone is coarsely grained and not very coherent; and the coal bands, which are accompanied by shales, are of variable thickness. The dip is very small, and to the S.E., but the stratification is totally unconformable with the graywacke. Pilsen is situated on a little stream, which unites close to the town with the Beraun; and the eastern limit of the sandstone seems to be a small tongue of coarse grit, which reaches the Beraun, and exposes a bed of coal on its western bank. At that point, however, the graywacke comes in, having been brought up by a mass of trap.

Section 2. Radnitz to Rakonitz.—The direction of this section is nearly S. and N., Radnitz being about 12 miles east of Pilsen, and Rakonitz 20 miles east of Luditz. Radnitz stands upon an incoherent coal measure sandstone; and two bands of coal are worked a little south of the town. Beyond the sandstone rises a hill of graywacke shale, protruded, Mr. Ansted believes, by the agency of a mass of trap visible a short way off. To the north of Radnitz is

an abrupt hill of the shale, considered to have been also brought up by a fault; and on its northern face commences a broad valley formed of coal measures, and bounded at its further extremity by another hill of graywacke, likewise thrown up by a fault. Coal is worked on three sides of this hill. The graywacke continues thence for six or seven miles, when the coal sandstone again constitutes the surface for a short distance (2 miles), and, after another interval occupied by graywacke, reappears forming the country around Rakonitz.

Section 3. Zebrak to Ginetz.—This section refers to a more southerly part of the district, and traverses a portion of the coal measures situated south of that line of graywacke which extends from Pilsen to Prague, and separates, except at one point, the coal field connected with the two first sections, from the district about to be noticed. At Zebrak, the point just mentioned, the coal measures intersect the graywacke range, in consequence apparently of a fault; and the section commences at Zebrak in graywacke shale near the junction of the coal measures with the graywacke. These shales extend to Horzowitz, where they are overlaid unconformably by the coal sandstone, which constitutes the surface of the country for about two miles. At that point is a hill, on the summit of which occurs a cherty sandstone considered by Mr. Ansted to be the base of the coal measures and to have been forced up into its present position. The beds dip about 60° S.E., and rest apparently upon a very coarse, hard, red conglomerate, to which succeeds a vast development of shale, containing occasionally Trilobites. This division of the graywacke series, is at some distance, covered again by the conglomerate upon a change of dip, and then continues nearly three miles to Ginetz, with the strata moderately inclined to the N.E. At that town a band of limestone occurs reported to be rich in Trilobites.

Section 4. Przilep to Karlstein.—This section is parallel to the last, and crosses the line of country between Pilsen and Prague. Two or three tolerably thick beds of coal are worked near Przilep and supply Prague with fuel. Fossils also are not deficient. About 6 miles towards the north-west, other but inferior beds of coal are wrought; but towards the east, the coal thins out between lofty precipices of shale, which form a narrow gorge. Pursuing the line of section towards the south-east, the direction of the dip, and at no great distance from Przilep, the coal basin is shut in by the steep face of a hill. At this point, Mr. Ansted believes, that the lower beds of the coal measures are not only brought up, but are bent over the upper, because, though the dip of the strata is to the S.E. or in the direction of the section, yet, on the summit of the hill above mentioned, is exposed an excellent natural surface of chert; and in a quarry near the top the inclination of the beds is about 25° S.E. or in the regular dip of the coal measures; and in a narrow valley at the bottom of a somewhat rapid descent, the lowest division of the graywacke is exposed dipping S. or actually overlying the coal measures. This inversion of superposition, Mr. Ansted explains by

assuming, that the granite comes near the surface, and that by its agency the graywacke has been thrown into a trough, and its lowest beds so brought up as to be made to rest against inverted beds of the coal measures. Proceeding in the line of section, the author found in graywacke shale, portions of a *Trinucleus*, *Trilobites ornatus*. (Trans. Prague National Mus. Soc. 1833.*)

The graywacke shell extends with contorted strata to an anticlinal hill of limestone, beyond which occur broken and rotten shales, then limestone, next shales again, and lastly the picturesque limestone hill of Karlstein. Further south is a valley of graywacke bounded by an altered rock, which is succeeded by granite. The Karlstein limestone is stated to be identical with that at Ginetz, (see section 3) and the two other localities in the present line of section. It is of a pale blue colour, very hard, contains several species of *Orthocera* and *Trilobites*, and is of great economical importance. The recurrence of the same limestone at different points, Mr. Ansted explains, by supposing, that the granite in these cases, is also near the surface, and that a displacement of it bent the yielding shales, but snapt asunder a brittle band of limestone once continuous, the portions of which, not removed by subsequent operations, are exhibited at the points mentioned in the line of section; and that the consequences of these operations have been, a disturbance in the regular succession, and an exhibition of the beds in the following order: granite, altered rocks, newest graywacke with limestone, oldest graywacke, coal measures.

In conclusion, Mr. Ansted offers the following observations as the results of his examination of this portion of Bohemia. The graywacke series is imperfectly developed, presenting at only one spot a passage upwards into the carboniferous series, and no passage downwards into the graywacke, resting always unconformably upon it; the secondary rocks are also very imperfectly developed, the mountain limestone being absolutely wanting, and the only indications of beds newer than the coal measures being a red conglomerate, into which they pass upwards. The flora of the coal measures is however well known to be rich, and to have yielded near Radnitz the fossils described by Count Sternberg. A genus allied to *Scorpio* is also stated to have been found in them. The fossils of the graywacke are said to be not very numerous; but the *Trinucleus* appears to be abundant on the line of road between Prague and Pilsen; and in a gorge near Lodentz, about fourteen miles from Prague, is a quarry which yields shells and other organic remains; and on the opposite side of the road, near the same spot, similar fossils may be obtained. *Trilobites* occur at Ginetz, and *Orthocera* at Karlstein; and both these localities and the neighbourhood of Prague are mentioned as rich in organic remains. The *Trinucleus Caractaci* is stated to occur near Zebrak.†

* Impressions of shells were also found by the author in a grayish sand rock, a little nearer Prague; and the *Trinucleus* is found at Zebrak and Praskoles, on the south side of the high road about 10 miles south of Beraun.

† See the fossils of Caradoc sandstone, Silur. Syst. pl. 23, f. 1.

A letter was afterwards read, addressed to Dr. Buckland, P.G.S. by the Rev. John Gunn, and dated Dec. 21st, 1839.

This letter was accompanied by three paramoudras from the chalk near Norwich; and they had been selected by Mr. Gunn on account of one of them presenting a tuberculated exterior, a character which he states a paramoudra commonly assumes when it is in contact with horizontal lines of nodular flints; and the other two had been chosen because Belemnites and shells are imbedded in their substance. The letter contains some observations on the irregularities in the surface of the Norfolk chalk, and on the pipes or sand galls by which it is penetrated. With reference to these tubular hollows, Mr. Gunn refers to Mr. Lyell's description of them, read at the meeting of the British Association at Birmingham, but he calls attention to their being constantly filled in the district examined by himself, with sand, gravel, or crag, to the total exclusion of all materials belonging to the strata between the chalk and the crag; and he therefore infers, that the sand galls were not eroded during the eocene period, but that during that long period the Norfolk chalk was denudated.

The letter was also accompanied by some specimens from the boulders contained in the diluvial (drift) strata of Norfolk and Suffolk. Mr. Gunn is of opinion that these masses of rock indicate what were the strata that formed the shore against which the (so-called) diluvial waves washed; and that the masses were borne out to sea in a similar manner to the portions of cliff now annually destroyed by the waves. If the bottom of the present sea were raised, he says it would present features analogous to those of the crag and diluvial formations of Norfolk and Suffolk.

XXXIV. *Intelligence and Miscellaneous Articles.*

PHENOMENA OF CALEFACTION.

M. BOUTIGNY has read a paper before the Academy of Sciences on Calefaction, by which term he designates the singular phenomenon presented by water when drops of it are thrown upon a very hot metallic surface.

It has generally been supposed that this effect is produced only at a very high temperature, but M. Boutigny finds that it occurs in a lead crucible, and consequently below 612° Fahrenheit.

M. Boutigny has observed also that æther gradually dropped into a platina crucible nearly red-hot, calefies as well as water, that is to say, the mass becomes round, without the occurrence of any appearance of ebullition, is afterwards rapidly agitated, and does not seem to wet the crucible. The quantity, however, goes on diminishing, but much less rapidly than if the vessel were cold. During this slow evaporation, a very irritating vapour arises, which does not at all resemble that of æther, but which in smell greatly resembles aldehyde, and of this the author supposes it to consist; the presence of air appears to be necessary to the production of this vapour. The commissioner to whom M. Boutigny's paper was

consigned, made an interesting observation: having immersed a piece of litmus paper into the crucible to try whether the vapour was acid, he observed that the part immersed retained its colour, whilst that which was even with the orifice of the crucible became evidently red. The temperature was therefore higher in this place, and it is to be presumed that slow combustion took place analogous to that which occurs in the interesting experiments of Dobereiner.

Anhydrous sulphurous acid presented M. Boutigny with phenomena still more remarkable than æther; he found that when a little of this acid dropped upon a platina capsule heated almost to redness, the drops were strongly agitated, became round, immoveable and opalescent, and seemed even to crystallize. The small spheroid when placed on the hand produced a sensation of cold.

M. Boutigny was of opinion, that in this case the sulphurous acid suffered so great a diminution of temperature that it solidified. The commissioner rejects this explanation, and is satisfied with admitting, that the acid under these circumstances evaporating more slowly than in the open air, produces nevertheless, by this slow evaporation, so considerable a degree of cold as to congeal the moisture of the surrounding air, and to become hydrated. This explanation is apparently confirmed by the fact, that if the small solid globule be rapidly projected into a tube, and it be immediately corked, the globule disappears, but leaving in the place which it occupied a dew, that remains even when the tube is uncorked.

M. Boutigny is of opinion that the phenomena described may be connected with the explosions in steam-boilers, and he is still occupied with the subject, and has made a great number of experiments with different liquids, and particularly with alcohol of different degrees of strength, with æther, oil of turpentine and lemons, and with alkaline and acid solutions.—*Journal de Pharm.*, Mai, 1840.

HYDROMELLONIC ACID.

Discovered by L. Gmelin. Prepared by dissolving mellonuret of potassium in boiling water, and adding to the solution hydrochloric, sulphuric, or nitric acid. A dirty white gelatinous precipitate which dries to a yellow powder, the hydrated hydromellonic acid. It is slightly soluble in cold, more freely in hot water, has a slightly acid reaction, and is not decomposed by hydrochloric and nitric acids.

Formula $C_6N_4 + H$; eq. = 94.32. *Turner's Elements of Chemistry*, p. 796.

CHROMIC ACID.

M. J. Fritzche prepares chromic acid by the careful addition of concentrated sulphuric acid to a hot concentrated solution of bichromate of potash; a crimson bulky precipitate is obtained, which is dried first by heat, and afterwards *in vacuo*. This is entirely chromic acid, which is to be washed to get rid of the mother water, and of the sulphuric acid which adheres to it. The author could

not obtain the compound of sulphuric and chromic acids described by M. Gay-Lussac in the 16th volume of the *Ann. de Chim. et de Phys.*, and he is inclined to question its existence.—*L'Institut*, No. 341.

COMPOSITION OF CRYSTALLIZED PHOSPHORIC ACID.

In the opinion of Prof. Graham, phosphoric acid may combine with water in three different proportions, and form a phosphate, biphosphate, triphosphate of water. The existence of these compounds has hitherto been hypothetical, except the first, which appears to be vitrified phosphoric acid. The analysis of crystallized phosphoric acid has not yet been attempted, or at any rate not yet published. M. Peligot has attempted to supply this void in the history of phosphoric acid, by analysing crystals which slowly formed spontaneously in bottles filled with syrupy phosphoric acid. One of these bottles contained two very distinct crystalline layers. The upper crystals were transparent and hard; the lower ones were soft, and had the appearance of sugar of honey.

The crystals, separately collected, were dried *in vacuo*, on plates of absorbent porcelain; the quantity of water was determined by calcining them with oxide of lead. According to M. Peligot's analyses, the upper crystals contained from 27 to 28 per cent. of water, and the lower crystals 22 to 23 per cent.

The extreme avidity of these crystals for water, renders a precise analysis of them very difficult; if, as theoretically supposed by M. Peligot, hydrated phosphoric acid containing three equivalents of water ought to contain 27.4 per cent., and the bihydrate 20.1 per cent., it will appear probable that the crystals examined were in fact these two hydrates. Their properties also corroborated this opinion; for the most hydrated acid, when saturated with ammonia, precipitated silver of a yellow colour, while the second precipitated it white.—*Journal de Pharm.*, Juin, 1840.

DETECTION OF ALCOHOL IN ESSENTIAL OILS.

For the above purpose M. Borsarelli employs a small cylindrical tube closed at one end; this is two-thirds filled with the oil, and there are dropped into it small pieces of chloride of calcium, which are quite dry, and free from powder; the tube is then closed, and heated in a water-bath to 212° for four or five minutes, taking care to agitate it occasionally, and to allow it to cool slowly.

If the essential oil contains a notable proportion of alcohol, the chloride dissolves entirely, and forms a liquid stratum, which occupies the lower part of the tube, while the essential collects in the upper. If the oil contains only a very small portion of alcohol, the fragments of chloride of calcium effloresce, lose their form, and unite at the bottom of the tube into a white adhesive mass; when it is quite pure the pieces of chloride suffer no change, even in their form.

It is proper to observe, that in trying an essential oil it is right to employ but a very small portion at first, and to add successive portions gradually; otherwise, if the proportion of alcohol be very small, it may be absorbed by the chloride without sensibly altering it, and even without showing its presence. It is easy when the operation is over to determine the proportions of a mixture of alcohol and essential oil, by comparing its weight or volume with that of the pure oil which floats upon the alcoholic solution of the chloride.

The same process, the author states, may be employed for determining the quantity of alcohol which aether contains; but the tube should be longer, and not too perfectly closed.—*Journal de Pharm.*, Juin, 1840.

ECONOMICAL PREPARATION OF ACETONE.

M. Zeise of Copenhagen observes, that when a considerable quantity of acetone is required, the methods usually employed are too expensive, and he recommends the following process: mix thoroughly one part of well-powdered quicklime with two parts of powdered crystals of acetate of lead. Soon after the mixture is made, the lime usually begins to heat violently; but as no smell of acetone is perceptible, there is no sensible loss of it; it is better to put the mixture into the distillatory apparatus before this heating occurs, because afterwards the mass is so great that the introduction is more difficult. M. Zeise states that he has not found it advantageous to attempt getting rid of this circumstance by employing slacked lime; for in this case dried acetate must be used to prevent the product from containing too much water, and the drying of the acetate is more troublesome than powdering the lime.

The iron bottles in which mercury is imported, suit for this operation extremely well; four pounds of the acetate may be operated upon at once; the bottle is placed almost horizontally in the furnace, but so that the opening is rather raised; to this a slightly curved short tube is adapted, and luted with a mixture of clay, chalk and salt, and this enters into a glass tube sufficiently large, properly curved and surrounded with a tin-plate pipe, in which an ascending current of water is kept up; and lastly, to this a receiver surrounded with ice is attached. The heat is slowly raised, and it is only towards the end of the operation that it is increased to redness.

The product is a mixture of acetone, a small quantity of water, two oily substances, which are less volatile than acetone, one of which is probably the dumasine of Kane.

From this product pure acetone is obtained by dissolving chloride of calcium in it, distilling the solution in a water-bath, until at 212° nothing more passes over; the product is to be again treated with chloride of calcium, and three-fourths being distilled will be found to be pure acetone.

On adding water to the residue, the oily bodies are separated from the acetone, which dissolves in the water and is separated from it by chloride of calcium.

From about 8 pounds of crystallized acetate of lead, M. Zeise obtained by the process now described from 10 to 11½ ounces of perfectly pure acetone.—*Ann. de Chim. et de Phys.*, t. lxxii.

CARBURET OF PLATINA.

M. Zeise observes that the formation of a compound of carbon and platina has been hitherto ineffectually attempted; he has, however, succeeded in procuring by means of decomposing acechlor-platina by heat.

When acechlor-platina is heated in a retort, to which a receiver is adapted, with a bent tube, it begins to decompose and yield a little gas at about 419°; at 464° the disengagement of gas is plentiful, and a brown liquid distils. This continues with the gradual increase of the heat up to 527°, a little colourless liquid coming over from time to time; at 572° much gas and liquid were produced; when this heat ceased to produce any effect, the retort was heated to redness in the sand-bath; this occasioned a copious disengagement of gas; so that the quantity obtained at this period was greater than all previously procured, but the quantity of liquid was much smaller; when neither gas nor liquid was obtained, the operation was stopped, and the residue cooled out of contact with the air.

The distilled liquid contained so much hydrochloric acid that it smoked in the air, and when mixed with water it gave an oily liquid which floated. Its odour was resinous and æthereal: the volume of the liquid was considerably diminished by treatment with water.

The gas was a mixture of hydrochloric acid and an inflammable gas, which was probably protocarburet of hydrogen, and there were traces of carbonic acid. The residue was black, slightly coherent, and free from metallic particles. The slowness with which it burnt, indicated that it was a chemical compound of carbon and platina.

In the first experiment 100 parts of acechlor-platina yielded 60·0107 of carburet of platina, and in the second 60·708; the mean is 60·362.

On the supposition that it is a bicarburet of platina, 100 parts of acechlor-platina ought to have yielded 60·347 parts of carburet. Then as 100 parts acechlor-platina, and consequently 60·362 of carburet of platina, contain 53·692 parts of platina, we have for 100 parts of carburet of platina,

Carbon	11·041
Platina.....	88·959

100

Calculation gives 11·029 carbon, and 88·971 platina; it retained scarcely a trace of chlorine. Carburet of platina may also be obtained by heating to redness a mixture of acechlor-platina and hydrate of lime.—*Ann. de Chim. et de Phys.*, t. lxxii.

SULPHATE OF CARBYLE.

M. E. Magnus states, that when anhydrous sulphuric acid is absorbed by absolute alcohol, there are formed, under the influence of particular circumstances, white silky crystals in the alcohol; they are a sulphate of carburetted hydrogen, to which M. Magnus gives the name of sulphate of carbyle, from *carbo* and *hydrogenium*. The method adopted was that of exposing the alcohol in a small tube to anhydrous sulphuric acid in a stopped receiver. Crystals are rarely formed immediately; the tube must be put into a second receiver containing sulphuric acid, and sometimes even into a third. For the sulphuric acid in the receiver also absorbs alcohol, which causes the cessation of the absorption of the acid by the alcohol which is contained in the tube. The formation goes on without the disengagement of sulphurous acid.

The crystals which form in the alcohol tube are, it is true, surrounded with fuming acid, but M. Magnus succeeded in isolating them. In pouring off the liquid acid which filled the interstices, they yield abundant vapours: they rapidly attract humidity from the air and deliquesce; to dry them they were placed upon a plate of baked clay, slightly heated in the air-pump over sulphuric acid until they yielded no more vapour.

When æther was employed instead of alcohol, no crystals were obtained; it is possible, however, that under some circumstances they might be formed; the experiment confirms the previous statement of M. Magnus, that when æther is absorbed by anhydrous sulphuric acid, heavy oil of wine is always produced, which is never the case with absolute alcohol. This may be regarded as one of the strongest objections against the opinion that alcohol is a hydrate of æther; for if this were the case, the sulphuric acid ought to remove the water, and then act upon it as if it were æther, which evidently does not occur.

When these crystals are cautiously heated, they fuse without decomposition, and on cooling become a crystalline mass. They combine with water and alcohol with the disengagement of heat, and by evaporation they do not separate from the solution. The aqueous solution, saturated with barytes, yields soluble salts with a little sulphate of barytes. The quantity of sulphuric acid obtained was variable, and appeared to be eliminated by the action of the water.

The soluble barytic salts are ethionate of barytes and a little isethionate, and it appears to be the conversion of ethionic into isethionic acid, which produces the sulphuric acid.

By analysis sulphate of carbyle appears to be composed of

Sulphuric acid.....	84.930
Carbon	12.955
Hydrogen	2.115

100

It is therefore a sulphate of carburetted hydrogen, or 1 equivalent

of sulphuric acid 40 + 1 equivalent of carburetted hydrogen 7, similar to that formed by the action of anhydrous sulphuric acid on olefiant gas.—*Ann. de Chim. et de Phys.*, t. lxxii.

ON HELLENIN. BY M. C. GERHARDT.

This substance, obtained from *Inula hellenium*, is to be distinguished from *inulin* yielded by the same root. It is readily obtained by treating the fresh root of the elecampane with alcohol of sp. gr. 0·837; when the excess of alcohol is separated by distillation, the concentrated liquor becomes milky by cooling, and deposits abundance of crystals. They are purified by redissolving in alcohol and recrystallizing. When the root is distilled with water, downy flocculi, which are very white pure hellenin, are obtained in the receiver; but the quantity is so small that it is better to employ alcohol.

Hellenin crystallizes in four-sided prisms; they are perfectly white, their smell and taste is extremely weak, and they are lighter than water. They are insoluble in water, but very soluble in æther and alcohol, and these solutions are precipitated by water. It dissolves also in all proportions, in essential oils and in creasote. It may readily be pulverized when it is rendered impure by the resin, which always exists with it in the root. Its fusing point is about 161° Fahr.; it boils at 527° to 536°, and volatilizes before it boils, exhaling a very weak odour. At this temperature, however, it is more or less altered, so that the density of its vapour cannot be ascertained.

When hellenin is fused at a gentle heat, it recrystallizes in a mass on cooling; but if the heat be continued for some minutes, the mass on solidifying does not possess a crystalline texture, but resembles resin in appearance. The caustic alkalies do not decompose hellenin, even when heated, a property which it possesses in common with several substances, such as camphor, the oil of aniseed, mint, &c. On heating it in a solution of potash, it first fuses and eventually dissolves, and on the addition of hydrochloric acid the hellenin is precipitated without alteration. When heated with hydrate of potash, a great part of it is volatilized, whilst another portion is carbonized; on dissolving the mixture afterwards in water a slightly brown coloured liquid is obtained, which becomes slightly turbid on the addition of acids.

Acids act upon hellenin as they do upon the greater part of the essential oils; concentrated sulphuric acid dissolves it at common temperatures, forming a red coloured solution, without the evolution of any sulphurous acid, provided no heat be employed; nevertheless after a considerable time the mixture becomes black, as if acted upon by heat. The solution then contains a certain quantity of a peculiar acid, to which M. Gerhardt has given the name of sulpho-hellenic acid.

When hydrochloric acid gas is brought into contact with hellenin, it absorbs a large quantity of the gas, and a liquid of a violet co-

lour is formed; when exposed to the air it exhales hydrochloric acid.

Nitric acid of middling strength dissolves hellenin without the evolution of any hyponitrous acid, and water precipitates it unaltered; when the mixture is heated the hellenin is converted into what M. Gerhardt calls *nitro-hellenin*.

Concentrated acetic acid dissolves hellenin; the solution is colourless, and deposits by evaporation unaltered crystals of this substance; water precipitates the solution.

Anhydrous phosphoric acid acts upon hellenin as it does upon camphor, converting it into a carburetted hydrogen, which M. Gerhardt has named *hellenene*.

Cold chlorine gas does not act upon hellenin, nor even when exposed to the direct rays of the sun; but when the mixture is heated, hydrochloric acid is disengaged and a resinous body is formed, in which a certain number of atoms of hydrogen are replaced by an equal number of atoms of chlorine; a drop of bromine produces with hellenin an effervescence of hydrobromic acid; the product is of a yellow red, dissolves in alcohol, and is precipitated from it by water. It is probably a compound analogous to that formed with chlorine, and the author terms it *hydrochlorate of chlorehellenin*.

Bichloride of tin and protochloride of antimony, the latter in a state of fusion, colour hellenin of a deep red colour, exactly like concentrated sulphuric acid.

When distilled with lime, hellenin yields a yellow inflammable liquid; it is neutral, does not mix with water, and in smell resembles acetone.

The analysis of hellenin performed by M. Gerhardt agrees nearly with that of M. Dumas, and he considers it as constituted of

30 atoms of carbon....	1146·6	77·92
20 atoms of hydrogen..	124·8	8·41
2 atoms of oxygen ..	200·0	13·67
	<hr/>	<hr/>
	1471·4	100

In composition it more nearly approaches creasote than any other substance, which, according to Ettling, contains

Carbon..	77·42
Hydrogen.....	8·12
Oxygen.....	14·46—100

Ann. de Chim. et de Phys., t. lxxii.

CHEMICAL AND CONTACT THEORIES OF VOLTAIC ELECTRICITY.

After giving an extract from Professor Faraday's Sixteenth Series of Researches, of which an abstract appeared in Lond. and Edinb. Phil. Mag., vol. xii. p. 122, M. de la Rive remarks, "The question of which Mr. Faraday treats in the memoir from which the foregoing extract is given, is the subject of a warm controversy at the present time, especially in Germany, where the partizans of the voltaic theory of contact are numerous. The authority of Mr. Faraday, and the clear and decisive manner in which he declares himself in favour of the chemical theory, are of great weight; the

numerous investigations which this philosopher has made in electricity, and his great practical acquaintance with voltaic apparatus, give great value to his opinions on the origin of voltaic electricity.

"In beginning the memoir here alluded to, he has done me the honour to mention that I am one of those who have most strongly pleaded for fifteen years in favour of the chemical theory; he has repeated most of the experiments upon which I have rested my opinion, and he has found them correct. I hope soon to be able, in like manner, to take up the experiments lately made in Germany in favour of the theory of contact, in order to show that they are in no way contrary to the chemical theory, and to add to this theory some new direct proofs. At the meeting of the *Société de Phys. et d'Hist. Nat. de Genève*, on the 21st of May last, I read a memoir on this subject, which will appear in one of our earliest Numbers."—
A. DE LA RIVE, *Bibliothèque Universelle*.

HYDROMELLONIC ACID AND METALLIC OXIDES.

The hydromellonic acid is decomposed by metallic oxides into a metallic mellonuret and water; it decomposes the carbonates both in the dry state and in solution, and the iodides and bromides on fusion. Its compounds with the alkaline metallic oxides and with the earths are insoluble in water.

Mellonuret of Potassium.—Prepared by fusing sulphocyanuret of potassium in a porcelain crucible at a red heat, and adding mellon as long as an evolution of sulphuret of carbon and sulphur is remarked. A brown opaque glassy mass is obtained, which dissolved in boiling water yields, as the solution cools, hydrated crystals of mellonuret of potassium. It may also be formed by fusing 5 parts of chloride of antimony (butter of antimony) with 8 parts of sulphocyanuret of potassium, and removing by boiling water the soluble portions of the residue after the escape of the sulphur and the sulphuret of carbon. It is also formed as a secondary product in the process for the preparation of the sulphocyanuret of potassium; it is present in the solution in small, but in the residue in larger quantity, from which it may be removed by boiling water.

Prop.—Crystallizes from water in colourless fine needles, which unite into dense flakes; a concentrated solution congeals to a soft white mass, which is with difficulty dissolved by cold water; the crystals contain water of crystallization, which they lose at a high temperature; they then fuse without loss of weight to a clear yellow glass. The solution is tasteless, and precipitates all earthy and metallic salts.

By fusing sulphocyanuret of potassium with mellon, the sulphocyanogen is liberated, and is instantly decomposed by the high temperature into sulphuret of carbon, sulphur, and mellon. By fusing 1 eq. chloride of antimony with 4 eq. sulphocyanuret of potassium, there are formed 3 eq. chloride of potassium 3 KCl, 1 eq. sulphuret of antimony Sb_2S_3 , 2 eq. sulphuret of carbon $2CS_2$, 1 eq. mellonuret of potassium KC_6N_4 , and 1 eq. of free sulphur. By fusing ferrocy-

anuret of potassium with sulphur, the sulphocyanuret of potassium, and sulphocyanuret of iron, are formed; 4 eq. of the latter decompose into 4 eq. sulphuret of iron 4 Fe S , 2 eq. sulphuret of carbon 2 CS_2 , and 1 eq. of mellon, which as soon as it is formed decomposes 1 eq. of sulphocyanuret of potassium into mellonuret of potassium and sulphocyanogen; the latter is further decomposed into sulphur, sulphuret of carbon, and mellon.

Formula $\text{K} + \text{C}_6\text{N}_4$; eq. = 132.47. *Turner's Elements of Chemistry*, pp. 797, 798.

CYANILIC ACID.

By a long-continued boiling of mellon in dilute nitric acid, a solution is effected with the evolution of gaseous products, and the liquid yields on evaporation colourless, transparent, octohedral crystals; by re-solution in hot water, hydrated cyanilic acid in soft tabular crystals of a mother-of-pearl lustre are obtained. This acid has the same composition as the crystalline cyanuric acid; contains, like the latter, 4 eq. water of crystallization, which it loses at 212° , when it becomes opaque and falls to a white powder. By the destructive distillation it is converted into hydrated cyanic acid; by solution in sulphuric acid and caustic potassa into cyanuric acid. This acid has been but little examined. Its formation admits of explanation on the supposition that the elements of 1 eq. of mellon and 3 eq. of water give rise to 1 eq. cyanilic acid and 1 eq. ammonia, and the latter is in point of fact found combined with the nitric acid; according to this, its formation ought not to be dependent on the use of nitric acid alone.—*Ibid.* p. 798.

METEOROLOGICAL OBSERVATIONS FOR JULY, 1840.

Chiswick.—July 1. Overcast: boisterous. 2. Rain, with strong wind. 3. Cloudy and fine. 4. Very fine. 5. Cloudy: windy. 6, 7. Fine. 8. Fine: heavy rain. 9—12. Very fine. 13—17. Fine. 18. Overcast. 19. Cloudy: rain. 20. Heavy showers. 21. Very fine: rain. 22. Fine. 23. Cloudy. 24. Overcast and fine: rain. 25. Showery. 26. Cloudy: fine. 27. Fine. 28. Hazy. 29. Very fine. 30. Cloudy: rain. 31. Very fine.

Boston.—July 1, 2. Rain. 3. Stormy. 4. Fine: rain early A.M.: rain A.M. 5. Fine: rain A.M. 6. Cloudy: rain P.M. 7. Cloudy: rain early A.M.: rain P.M. 8. Cloudy: rain P.M. 9. Cloudy. 10. Cloudy: rain P.M. 11—13. Cloudy: rain A.M. and P.M. 14, 15. Fine. 16. Rain: rain early A.M. 17. Fine. 18, 19. Cloudy: rain P.M. 20. Fine. 21. Fine: rain P.M. 22. Fine. 23, 24. Cloudy. 25. Rain: thunder and lightning with rain P.M. 26. Cloudy. 27. Fine. 28. Cloudy: rain A.M. 29. Fine. 30. Cloudy. 31. Fine.

Applegarth Manse, Dumfries-shire.—July 1. Heavy rain A.M.: cleared up P.M. 2. Drizzling all day. 3. Heavy rain all day. 4. Fair till 4 P.M. then wet. 5. Showery: fair evening. 6. Rainy. 7, 8. Showery: thunder. 9. Fair all day. 10. Showery. 11. Warm: a single shower: thunder. 12. Very wet. 13. Fine dry day. 14. Wet afternoon. 15. Very wet all day. 16, 17. Occasional showers. 18. Fair till afternoon, then wet. 19. Rain early A.M.: cleared up. 20. Fair all day. 21. Heavy showers all day: thunder. 22. Fair all day. 23. Fair till evening, then rain. 24. Showery all day. 25. Showery afternoon. 26—30. Fair all day. 31. The same: a few drops P.M.

Sun shone out 29 days. Rain fell 22 days. Thunder 3 days.

Wind north $\frac{1}{2}$ day. North-north-east $\frac{1}{2}$ day. East-north-east 1 day. East 1 day. South-east $\frac{1}{2}$ day. South 4 days. South-west 8 days. West-south-west 3 days. West 7 days. North-west $2\frac{1}{2}$ days. North-north-west 3 days.

Calm 11 days. Moderate 12 days. Brisk 4 days. Strong breeze 2 days. Boisterous 1 day. Variable 1 day.

Days of Month, 1840. July.	Barometer.				Thermometer.				Wind.				Rain.		Dew point. Land: Roy. Soc. 9 a.m.	
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Self-register. Fabr. 9 a.m.	London: Roy. Soc.		Chiswick.	Boston.	Dumfries-shire.			
		Max.	Min.		9 a.m.	8½ p.m.			Max.	Min.				Fahr. 9 a.m.		Max.
1.	29.922	29.985	29.809	29.60	62.0	68.2	52.4	68	58	56	61	50	133	.01	...	58
2.	29.774	29.739	29.544	29.31	61.2	66.6	59.6	66	55	54	59½	46½30	...	58
3.	29.540	29.708	29.483	29.35	58.8	66.5	56.5	68	51	62	56	50½	.033	.16	...	58
4.	29.878	29.810	29.676	29.44	61.3	69.2	53.7	72	56	58	59½	47	.080	.10	...	54
5.	29.732	29.710	29.681	29.34	63.3	68.6	58.7	69	50	63	60	49	56
6.	29.810	29.767	29.434	29.20	62.6	75.3	53.3	67	49	62	59	48½	.016	.02	1.87	55
7.	29.634	29.718	29.536	29.00	59.7	71.7	50.6	71	51	53	61	48	.138	.83	...	55
8.	29.822	29.764	29.625	29.26	60.2	66.3	53.8	66	48	60	59	47	.025	.06	...	55
9.	29.878	29.928	29.798	29.19	59.4	67.3	51.3	68	48	60.5	59	47	.500	.46	...	53
10.	29.996	29.946	29.893	29.30	60.3	67.7	51.3	67	50	60	63	5005	...	53
11.	30.002	29.936	29.924	29.42	60.2	66.8	52.6	68	46	56	61	49	53
12.	30.012	29.949	29.905	29.43	58.3	69.2	52.2	65	42	55	57½	4804	...	54
13.	30.012	30.033	29.950	29.46	55.7	62.7	49.4	62	41	56	59½	4341	...	53
14.	30.316	30.269	29.942	29.70	59.4	70.5	50.2	74	55	59	59½	40½07	...	53
15.	30.304	30.235	30.137	29.62	62.7	77.2	57.0	80	48	65.5	59	51½05	1.80	50
16.	30.152	30.101	29.919	29.50	66.5	74.3	56.0	79	46	58	61	51	54
17.	29.998	29.909	29.889	29.36	62.7	74.6	52.8	75	56	62	58½	49½10	...	56
18.	29.778	29.735	29.717	29.18	63.7	69.7	58.0	70	56	64	61	4503	...	55
19.	29.700	29.662	29.470	29.05	63.0	68.6	58.7	69	49	61	63	5306	...	60
20.	29.566	29.511	29.441	29.30	63.7	70.0	53.2	69	51	63.5	63½	53	.091	.09	...	60
21.	29.630	29.646	29.563	29.00	63.7	70.0	55.3	74	47	63.5	64	50½	.066	.10	...	58
22.	29.820	29.844	29.757	29.22	61.4	70.2	52.7	69	49	60	62	50½	.305	.04	...	59
23.	30.032	30.013	29.981	29.43	59.7	72.4	53.5	65	51	56	59	5001	1.41	56
24.	30.066	29.993	29.906	29.50	60.7	64.5	55.0	66	56	58	58	49	52
25.	29.814	29.774	29.646	29.24	58.5	67.4	58.5	67	54	58	64	51	54
26.	29.604	29.739	29.627	29.16	58.8	70.2	56.3	66	54	61	66	53	.016	.22	.07	56
27.	29.956	29.926	29.745	29.37	61.0	62.4	55.3	70	49	64	64	50½	.291	.05	.24	58
28.	30.084	30.078	30.019	29.47	61.7	71.4	55.6	74	58	63	65	55	.105	.01	...	58
29.	30.260	30.171	30.134	29.63	60.4	71.8	60.2	75	54	65	66	45½	57
30.	30.148	30.095	30.029	29.47	65.4	72.0	60.2	69	51	62.5	65	58½03	...	57
31.	34.210	30.159	30.126	29.59	62.3	73.2	58.7	70	45	60	61	49	.016	.05	...	61
Mean.	29.921	29.898	29.780	29.30	61.5	69.8	54.9	69.35	50.77	59.9	61.1	49.4	Sum.	1.68	2.76	Mean.
				29.650									1.815			56.

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[THIRD SERIES.]

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XXXV. *Comparative Measure of the Action of two Voltaic Pairs, the one Copper-zinc, the other Platina-zinc.* By M. JACOBI*.

I HAVE the honour to communicate to the Imperial Academy of Sciences, the result of my comparative experiments, concerning the force of two different voltaic pairs with partitions, the one copper-zinc, charged with sulphate of copper and sulphuric acid dilute with six parts by volume of water; the other platina-zinc, and charged, according to the recommendation of Mr. Grove, with concentrated nitric acid, and with the same diluted sulphuric acid. The first pair, copper-zinc, had 36 square inches of surface, the pair platina-zinc had only $2\frac{1}{2}$ square inches. To measure the force of the current, I employed M. Becquerel's electro-magnetic balance. This instrument is valuable for accurate measures, provided the helices be so disposed as to be able to fulfil the conditions of stable equilibrium, which necessarily ought to exist in a balance. This is attained by making the repulsion only act between the magnetic bars and helices. For this purpose, one of the helices ought to be fixed below, the other above the said bars. This last helix is traversed by the rod, by which the bar is suspended from the beam of the balance. It is still necessary that a correction be adapted to the currents measured by the balance. This correction, of which other synchronous investigations have shown the necessity, is in proportion to the square of the force of the current. For

* Read before the Imperial Academy of Sciences of St. Petersburg, on the 31st of January, 1840.

let k' be the actual current, k the current measured by the balance, we have the equation

$$k = k' - y k'^2,$$

from which is deduced $k' = \frac{1}{2y} (1 - \sqrt{1 - 4ky})$. For my balance I have found, by a series of observations, $y = 0.00004228$ (*Bulletin Sc. de l'Acad. Sc. tom. v. p. 375.*).

The following table contains the experiments made with the voltaic combinations in question. The first column contains the resistances, L , of the helices which serve as conjunctive wire, and which had been found by other experiments; the two other columns contain the forces of the effective currents, or of the currents measured in grammes, and corrected according to the formula above indicated.

L	Force of the pair, Copper-zinc.	Force of the pair, Platina-zinc.
23.1	gr. 0.380	gr. 0.395
135.3	0.097	0.135

Let A, A' be the electro-motive forces, λ, λ' the resistances of the pair itself, we shall have, according to Ohm's formula, the four following equations:

$$\frac{A}{\lambda + 23.1} = 380$$

$$\frac{A'}{\lambda' + 23.1} = 395$$

$$\frac{A}{\lambda + 135.3} = 97$$

$$\frac{A'}{\lambda' + 135.3} = 135,$$

whence $A = 14610$, $\lambda = 15.35$, $A' = 23000$, $\lambda' = 35$; or taking λ as the unit of surface, which is here 36 square inches,

$\lambda' = \frac{35 \times 2.5}{36} = 2.4$. Let s be the total surface of a pile,

z the number of pairs, C the force of the current, L any resistance, we have $C = \frac{z A s}{z^2 \lambda + L s}$. From this equation we

find, that the maximum of force is obtained, if the pile be arranged so that $\frac{z^2 \lambda}{s} = L$, i. e. that the total resistance of the

pile shall be equal to the resistance of the conductor, whatever its nature may be, this resistance being one which is interposed in the circuit, and not belonging to the pile. As for

other arrangements than those which correspond to the maximum of effect, there is no constant relation between different voltaic combinations; we can only compare them, and judge of their relative preferableness by referring them to this maximum of action. We have, by eliminating z , the equations

$$C (max) = \frac{A \sqrt{s}}{2 \sqrt{\lambda} L} = \frac{A' \sqrt{s'}}{2 \sqrt{\lambda'} L},$$

whence we deduce, by substituting the numerical values above found for $A, A', \lambda, \lambda' \dots \dots \dots s' = s \cdot 0.06$

and with reference to the number of pairs $z' = z \cdot 0.6$;

that is to say, *it requires only a pile of 6 square feet of platina to replace a pile of 100 square feet of copper; or with reference to the number of pairs, 6 pairs of platina, each of a square foot of surface, will produce the same effect as 10 pairs of copper, each of which presents a surface of 10 square feet.* This eminent superiority of platina, as in Mr. Grove's combination, is verified by many experiments on a large scale.

XXXVI. *On the Application of Huyghens's Principle in Physical Optics.* By R. POTTER, Esq., B.A.*

IN the present paper I propose to examine some of the consequences of the method at present followed in developing the results of the undulatory theory of light, which consists in considering elementary waves, having their origin in some previous positions of the main waves, as the cause of these latter in succeeding positions. In the *Mémoires de l'Acad.* for 1821 and 1822, Fresnel announces this method in the following terms:—

“ Application du Principe d'Huygens aux Phénomènes de la Diffraction.

“ Ce principe que me paraît une conséquence rigoureuse de l'hypothèse fondamentale, peut s'énoncer ainsi: *Les vibrations d'une onde lumineuse dans chacun de ses points peuvent être regardées comme la somme des mouvemens élémentaires qu'y enverraient au même instant, en agissant isolément, toutes les parties de cette onde considérée dans une quelconque de ses positions antérieures.*”

Again, in speaking of the analytical process, he says, “ La recherche de la loi suivant laquelle leur intensité varierait autour de chaque centre d'ébranlement, présenterait sans doute de grandes difficultés; mais heureusement nous n'avons pas besoin de la connaître; car il est aisé de voir que les effets

* Communicated by the Author.

produits par ces rayons se détruisent presque complètement dès qu'ils s'inclinent sensiblement sur la normale, en sorte que ceux qui influent d'une manière appréciable sur la quantité de lumière que reçoit chaque point P peuvent être regardés comme d'égale intensité."

In the application of the above principle, we take the origins of the elementary waves on any proposed surface as that of a reflecting or refracting substance, or an aperture, without its being necessary that this surface should coincide with any one wave surface as it arrives.

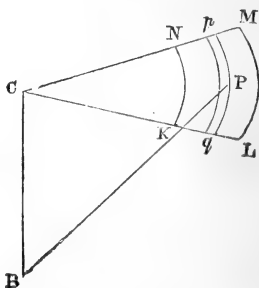
There are only a few cases in which the integration for the whole vibration of a particle can be effected directly; in the following, however, the integration involves no difficulty, and they suffice for proving the discordance of the results of the principle with acknowledged facts. They show that the labour which has been expended in investigating more complicated cases, might with ordinary caution have been saved.

The integration is readily performed when a series of plane waves fall on a plane reflector, or an aperture parallel to their surfaces, the reflector or aperture being of one of the circular forms enumerated below, and the particle whose vibration is required, being in the normal to such surfaces through the centre of the circular arcs. These forms comprise a circular aperture of any radius, an annulus contained between two circles, a circular sector, and the quadrilateral figure bounded by two radii, and two circular arcs: this latter form approximates to a rectilinear parallelogram, when the angle between the bounding radii is small, and the radii of the circular arcs are large.

The integration for apertures or reflectors of the same forms, is readily performed also when light diverges from a luminous point in the normal line through the centre of the arcs; and the particle whose vibration is required is equally distant from the surface in the same line, and on the opposite or same side with the luminous point, according as an aperture or a reflector is considered.

Let us commence with a series of plane waves falling directly on a quadrilateral aperture KLMN, bounded by the two concentric circular arcs KN, LM, and the radii CM, CL containing the angle $MCL = \theta$.

Let B be the position of the particle whose vibration is required situated anywhere in



the line B C which is perpendicular to the plane of the aperture, and let C B = h .

Let $p P q$ be any element of the aperture, whose breadth = δr , and distance C p or C q from C = r , therefore its distance from B = $\sqrt{r^2 + h^2}$ and its area = $\theta r \delta r$.

Then, by the principle under discussion, we have the displacement of the particle at B caused by this element, proportional to

$$\frac{\text{area of element}}{\text{distance B P}} \sin \left\{ \frac{2\pi}{\lambda} (vt - \text{B P}) \right\} \\ = \frac{a \theta r \delta r}{\sqrt{r^2 + h^2}} \sin \left\{ \frac{2\pi}{\lambda} (vt - \sqrt{r^2 + h^2}) \right\},$$

where a is some number.

Integrating for the whole vibration we have

$$a \theta \int_r \frac{r}{\sqrt{r^2 + h^2}} \sin \left\{ \frac{2\pi}{\lambda} (vt - \sqrt{r^2 + h^2}) \right\} \\ = \frac{a \theta \lambda}{2\pi} \cos \left\{ \frac{2\pi}{\lambda} (rt - \sqrt{r^2 + h^2}) \right\} + C \text{ between the limits} \\ \left. \begin{array}{l} r = r_1 \\ r = r_2 \end{array} \right\} \\ = \frac{a \theta \lambda}{\pi} \sin \left\{ \frac{\pi}{\lambda} (\sqrt{r_2^2 + h^2} - \sqrt{r_1^2 + h^2}) \right\} \\ \sin \left\{ \frac{2\pi}{\lambda} (vt - \frac{1}{2} \sqrt{r_2^2 + h^2} - \frac{1}{2} \sqrt{r_1^2 + h^2}) \right\},$$

which gives the intensity of the light at B

$$= \frac{a^2 \theta^2 \lambda^2}{\pi^2} \sin^2 \left\{ \frac{\pi}{\lambda} (\sqrt{r_2^2 + h^2} - \sqrt{r_1^2 + h^2}) \right\}.$$

The intensity becomes a maximum and = $\frac{a^2 \theta^2 \lambda^2}{\pi^2}$, or equal to $4 a^2 \lambda^2$ when $\theta = 2\pi$.

$$\text{If} \quad \sqrt{r_2^2 + h^2} - \sqrt{r_1^2 + h^2} = \frac{2n + 1}{2} \lambda,$$

where n is any integer. This equation may be satisfied by an indefinite number of values of r_1 and r_2 .

When r_1 and r_2 are very great, and h small, we have

$$r_2 - r_1 + \frac{h^2}{2} \left(\frac{1}{r_2} - \frac{1}{r_1} \right) + \&c. = \frac{2n + 1}{2} \lambda \\ = r_2 - r_1 \text{ nearly.}$$

If now θ be not large and $r_2 - r_1$ be only a small odd number of multiples of $\frac{\lambda}{2}$, the aperture will be nearly a parallelogram, and the maximum intensity $= \frac{a^2 \theta^2 \lambda^2}{\pi^2}$. But if in the original expression we make $r_1 = 0$, the aperture becomes the whole sector, and the maximum intensity, by giving r_2 the proper value, is $\frac{a^2 \theta^2 \lambda^2}{\pi^2}$. This shows that at however great a distance from C such a quadrilateral aperture be situated, and however near to C the point B may be, the intensity ought to be the same as for the sector.

The result of the principle is therefore that light ought to bend into the shadows of bodies to an indefinite extent, as sound is known to pass through all apertures, and bend round all obstacles.

It proves that the result Mr. Airy (Tract, page 270.) has obtained by an approximate method is not to be depended upon, and that the objection to the undulatory theory which was believed to have been removed remains in full force.

If it be said that these expressions involving λ^2 as a multiplier, must represent light of very feeble intensity, and therefore insensible, or nearly so, we shall see that we have the same small quantity in the expression for a large circular aperture.

If we make $\theta = 2\pi$, r_2 very large, and $r_1 = 0$, we have a large circular aperture, and the intensity

$$= 4 a^2 \lambda^2 \sin^2 \left\{ \frac{\pi}{\lambda} (\sqrt{r_2^2 + h^2} - h) \right\}$$

$$= 4 a^2 \lambda^2 \text{ when } \sqrt{r_2^2 + h^2} - h = \frac{2n+1}{2} \lambda;$$

and however great or small h may be, compared with r_2 , we see that there will be a succession of maxima and minima values for different values of h . This is at variance with the admitted properties of light, which it is allowed passes through large apertures without any change or diminution, or when diverging from a luminous point follows the law of the inverse square of the distance, except near the boundaries of the shadow.

If we compare the maximum intensity from an annulus, however narrow it may be, and however large the radii, with that from a large circular aperture, we see that *they are the same*; and the multiplier λ^2 would either show that only a very small quantity of light could pass directly through any large

circular aperture; or otherwise, if it were maintained that the constant a may be very large; then our former conclusions will be free from any objection that they represent inappreciable quantities of light.

A fact stated by M. Fresnel, which I have confirmed by a severe experimental examination, bears upon the point, of the effect of the limits of apertures. His experiments for the diffraction by a single edge of an opaque plate (see the before-mentioned memoir, page 429.) were, in fact, made with an aperture generally of a centimeter in breadth, whilst his luminous point and micrometer were in some measures distant about seven metres from each other. Notwithstanding the small breadth of the aperture compared with its distances from the luminous point and micrometer, yet we find him stating that the fringes formed by one edge were not affected by the other edge, and that his measures might be taken as if made with a single edge. The principle under discussion shows, that with the quadrilateral, sectorial or circular forms of apertures, the intensity should depend on the limits, however distant.

The same results arise when we take plane reflectors in place of apertures, only then the point B must be taken on the same side as that on which the waves are incident.

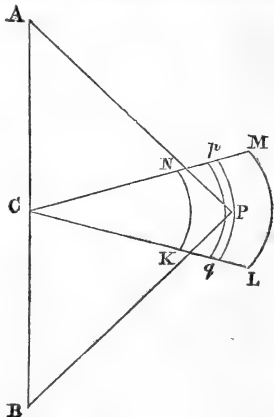
To discuss the second case, let A be the luminous origin in the line ACB perpendicular to the plane of the aperture, through the centre C . Let $AC = BC = h$, and the other parts as in the former figure.

We have now for the displacement of the particle at B, due to the element $p P q$,

$$\frac{a \theta r \delta r}{\Lambda P + P B} \sin \left\{ \frac{2\pi}{\lambda} (v t - (\Lambda P + P B)) \right\}$$

and the whole displacement

$$\begin{aligned}
 &= \frac{a \theta}{2} \int_r \frac{r}{\sqrt{r^2 + h^2}} \sin \left\{ \frac{2 \pi}{\lambda} (vt - 2 \sqrt{r^2 + h^2}) \right\} \\
 &= \frac{a \theta \lambda}{8 \pi} \cos \left\{ \frac{2 \pi}{\lambda} (vt - 2 \sqrt{r^2 + h^2}) \right\} + C \text{ between the} \\
 &\quad \text{limits } \left. \begin{matrix} r = r_1 \\ r = r_2 \end{matrix} \right\}
 \end{aligned}$$



$$= \frac{a \theta \lambda}{4 \pi} \sin \left\{ \frac{2 \pi}{\lambda} \sqrt{r_2^2 + h^2} - (\sqrt{r_1^2 + h^2}) \right\} \\ \sin \left\{ \frac{2 \pi}{\lambda} (v t - \sqrt{r_2^2 + h^2} - \sqrt{r_1^2 + h^2}) \right\}$$

and the intensity

$$= \frac{a^2 \theta^2 \lambda^2}{4^2 \pi^2} \sin^2 \left\{ \frac{2 \pi}{\lambda} (\sqrt{r_2^2 + h^2} - \sqrt{r_1^2 + h^2}) \right\}.$$

As in the former case, when we take h small and r_1, r_2 very large, with θ small, so as to form a quadrilateral aperture approximating very nearly to a parallelogram, we arrive at the same conclusion, that light, according to the principle under discussion, ought to pass through apertures, however *obliquely* situated with respect to its direction, and diverge into the shadow to an indefinite extent; the maximum intensity at B, being the same as if the aperture were the whole sector, and this holding, however near A and B may be to C.

If we take $A = 2 \pi$ and take r_2 very large, whilst r_1 is small, we have the case of the intensity in the centre of the shadow of a small circular disc, which it was found by approximate methods, ought to be the same as if the light passed uninterrupted; and M. Arago, having tried the experiment, found the result to accord. The complete investigation gives the intensity

$$= \frac{a^2 \lambda^2}{2^2} \sin^2 \left\{ \frac{2 \pi}{\lambda} (\sqrt{r_2^2 + h^2} - \sqrt{r_1^2 + h^2}) \right\},$$

which goes through a series of maxima and minima values for different values of h , when r_1 and r_2 are given; and not an uniform or slowly diminishing intensity along this line, as found by the approximate discussion.

There is another case which places the absurdity of the principle in a very striking point of view, which is the case of a large circular aperture, then $r_1 = 0$, and we have the intensity

$$= \frac{a^2 \lambda^2}{2^2} \sin^2 \left\{ \frac{2 \pi}{\lambda} (\sqrt{r_2^2 + h^2} - h) \right\} :$$

the maximum intensity is here dependent on h for its position, but not for its magnitude; that is, the maximum intensity at B is $\frac{a^2 \lambda^2}{2^2}$, however near A and B may be together, or however distant; contrary to the received and demonstrated principle, that the intensity of light diverging from a luminous origin varies as the inverse square of the distance.

Queen's College, Sept. 1840.

XXXVII. On Sulphocyanogen. By Mr. E. A. PARNELL*.

1. Its Composition.

WHILE engaged with an investigation of the action of alkalis on this substance (the results of which will be presently communicated), in which I was unable to account, in a satisfactory manner, for the production of a new acid, I was led to suspect the existence of hydrogen in sulphocyanogen; more especially as M. Liebig had obtained traces of water in its combustion by oxide of copper, which he then attributed to hygrometric moisture.

Before detailing my own results, I may state those which Liebig obtained with reference to this subject (*Ann. de Chim. et de Phys.*, tom. xli. p. 200.). Three-tenths of a gramme of sulphocyanogen, dried with great care *in vacuo*, afforded him in four combustions,

1.	·011	gramme of water.
2.	·017	— —
3.	·009	— —
4.	·016	— —

The mean of these gives 0·48 per cent. of hydrogen.

In repeating the analysis I have invariably obtained a much larger proportion of hydrogen. The sulphocyanogen examined was precipitated from the sulphocyanide of potassium by chlorine, and possessed all the characters of a pure substance: that used in the first and second analyses was kept on a sand-bath, at about 200°, for several hours, and afterwards for four hours in a water-bath at 212°.

1. 10·36 grains gave 0·85 grain of water equal to 0·91 per cent. of hydrogen.

2. 10·27 grains gave 7·49 carbonic acid, and 0·84 of water, or equal to 20·22 per cent. of carbon, and 0·90 of hydrogen.

To avoid all chance of error from the presence of hygrometric moisture, sulphocyanogen prepared by chlorine was dried in a Liebig's drying tube for some hours, by a nitre-bath at 242°. At this temperature a faint odour of cyanogen was perceived, but no other change.

11·02 of this gave 7·93 carbonic acid, and 0·96 of water, equal to 19·91 per cent. of carbon and ·96 of hydrogen.

To estimate the sulphur 10·11 grains were ignited with eight times as much nitrate and carbonate of potash; afterwards treated with nitric acid, diluted, filtered, and nitrate of barytes added. The sulphate of barytes amounted to 38·54 grains, which is equal to 52·59 per cent. of sulphur.

If the received equivalent of sulphocyanogen be doubled, and

* Communicated by the Author.

one equivalent of hydrogen added to it, making the formula $S_4 C_4 N_2 H$, the hydrogen will amount to .84 per cent. My estimations of the carbon and sulphur are then below the theoretical quantities. But before deciding on its constitution, an important question presents itself,—is the substance examined the radical of the sulphocyanides, or a product of the decomposition of that radical? To settle this point, the basic sulphocyanide of lead (the double sulphocyanide and oxide of lead), was first examined for hydrogen. It gave me 0.39 per cent., together with 4.20 per cent. of carbon. Now the hydrogen here is twice as much as it should be, supposing the salt to have the constitution $S_4 C_4 N_2 H + 2 Pb + 2 Pb O$, or the usual formula doubled, and one atom of hydrogen added to the radical. So it might be a hydrate of the above containing one equivalent of water; in which case it should contain 74.32 per cent. of lead. If, on the other hand, it be $Pb Scy + Pb O . H O$, it should contain 73.26 per cent. The carbon and hydrogen agree for either view. Liebig obtained (see memoir above-quoted) 74.958 per cent. of lead; two analyses gave me the following results.

1. 53.67 grains treated with nitric and a little sulphuric acid, gave 53.97 grains of sulphate of lead, equal to 39.58 of lead, or 73.74 per cent.

2. 59.98 gave 64.91 of sulphate, equal to 44.32 of lead, or 73.85 per cent.

As from these analyses the question was still undecided, I took the sulphocyanide of silver, which gave 7.20 per cent. of carbon, and only .05 of hydrogen, which is evidently due to hygrometric moisture; for if hydrogen existed in the radical, thus, $S_4 C_4 N_2 H + 2 Ag$, it should have contained .30 per cent. Thus it appears that the radical of the sulphocyanides does not contain hydrogen, and consequently, what has been regarded as sulphocyanogen, is a product of the decomposition of that radical. I will presently consider how it is produced.

It also appears that the basic sulphocyanide of lead is a hydrate (forming one of those few substances which contain six elements); probably, according to the formula $Pb Scy + Pb O H O$, containing

	Calculated.	Found.
Sulphur.....	11.38	
Carbon	4.33	4.20
Nitrogen	5.01	
Hydrogen34	.39
Oxygen	5.66	
Lead.....	73.28	73.78
	<hr/> 100.00	

To return to the composition of "sulphocyanogen." It is greatly to be regretted that we possess no data to decide on the formula of this substance, but its composition per cent. as given by ultimate analysis, which is far less satisfactory in an uncrystallized substance, as in the present instance, than in a crystallized body: and I do not know of any circumstance to guide us in fixing its equivalent.

The constitution which first presents itself as most probable, is to double the old formula, and add one equivalent of hydrogen to it, which would give sulphur 54.48, carbon 20.70, nitrogen 23.97, hydrogen 0.84 = 100.

But this does not exactly accord with the results of analysis. Taking the mean, we shall have as its composition per cent.,

Sulphur.....	52.59
Carbon	20.06
Nitrogen (calc.)..	23.23
Hydrogen.....	.92

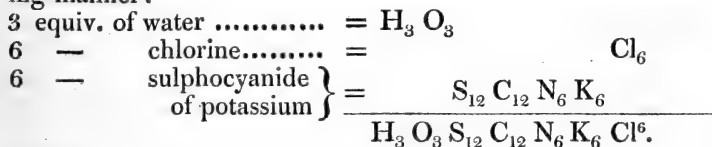
96.80

Here is then a deficiency of 3.2 per cent., which must be considered as oxygen. The formula with which this best agrees, and which I would adopt *provisionally*, is $S_{12} C_{12} N_6 H_3 O$, or

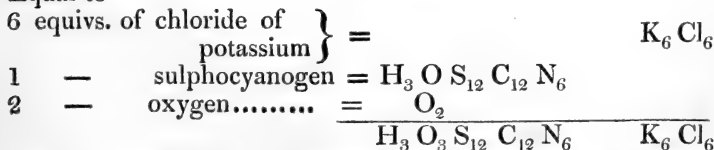
12 Sulphur	2414	53.27
12 Carbon.....	917.22	20.24
6 Nitrogen.....	1062.24	23.45
3 Hydrogen ...	37.43	.83
1 Oxygen	100.00	2.21

1 Equivalent... 4530.89 100.00

On this view of its composition, the reactions which occur in its production by chlorine may be explained in the following manner:



Equal to



Or six equivalents of sulphocyanide of potassium, six of chlorine, and three of water, become one of sulphocyanogen, six

of chloride of potassium, and two of oxygen. But what becomes of this oxygen? I have observed, that however slowly the chlorine be passed through the solution of sulphocyanide of potassium, an oxidizing action on the sulphocyanogen already formed takes place, even at the commencement of the operation; sulphuric and cyanic acids being produced. It does not appear how this action can be satisfactorily explained on the old view (a simple removal of potassium by chlorine), for it involves the decomposition of water by chlorine in a strong solution of sulphocyanide of potassium, or in fact, that water is more readily decomposed by chlorine than sulphocyanide of potassium. But on the view given above, an oxidizing action on sulphocyanogen already formed is easily explained; it is even essential, since no evolution of oxygen gas is to be noticed.

The production of this substance by nitric acid can be explained in a similar manner. Six equivalents of water, sulphocyanide of potassium, and nitric acid, are equal to six of nitrate of potash and six of hydro-sulphocyanic acid, or $S_{12} C_{12} N_6 H_6$, which with four equivalents of oxygen from the decomposition of another portion of nitric acid, become $S_{12} C_{12} N_6 H_3 O$, and $3 H_2 O$.

Four equivalents of oxygen of the atmosphere acting on six of hydro-sulphocyanic acid, produce the same effect.

With regard to the arrangement of the elements in this substance, it is obvious that many formulæ might with equal probability be selected. All that can be reasonably assumed on this subject is, that the carbon and nitrogen exist as cyanogen: it is also probable that the cyanogen and sulphur are more intimately connected with each other than either of them is with the oxygen or hydrogen; in which case this substance will be either a hydrate or an oxide of a hydruret of a sulphuret of cyanogen: but it appears probable that in all compounds which contain hydrogen united to a radical so as to form a hydruret, the hydrogen should be removed by chlorine, which in this instance is not the case. On another view the hydrogen (either wholly or in part) might be supposed to exist as an electro-positive or "zincous" element, but we do not find that it is displaced by metals, which certainly ought to be the case if such is the true constitution. But instead of adopting views of its constitution on insufficient data, it will be better in the present state of our knowledge of this substance to be satisfied with its empirical formula alone. The name of the substance will obviously require change, but I shall leave this to him to whom we are indebted for its discovery; and although I have been led to

observe an error in the conclusion at which he had arrived, it must not be forgotten that it is by his refined and beautifully simple method of analysis that I have been able to attain my results.

For convenience, I shall continue in the remainder of this paper to speak of the substance derived from the sulphocyanides as sulphocyanogen.

2. *Action of Alkalies on Sulphocyanogen.*

We are indebted to Wœhler and Liebig for all that is as yet known respecting the action of alkalies on this substance; but this subject was not studied so completely by them as it appears to deserve, nor so satisfactorily as other points connected with the sulphocyanides in their remarkable researches.

It appears from their experiments, that when sulphocyanogen is digested in solution of potash, a small portion is dissolved, and the remainder becomes redder, partly soluble in water, and partly in alcohol. After alcohol and water had been successively applied to this altered sulphocyanogen until nothing more was dissolved, a clear yellow substance remained, which Liebig considered as a higher degree of sulphuration of cyanogen. But his analyses of this substance do not sufficiently accord with any theoretical numbers to decide its constitution.

My own experiments certainly confirm the above as far as they go, but it would appear that heat had not been applied to the mixture of sulphocyanogen and alkali, but that it had, on the contrary, been kept at common temperatures; for if sufficient potash had been present and a gentle heat applied, the whole of the sulphocyanogen would have been dissolved, and converted into other substances.

When three parts of sulphocyanogen are digested with about four parts of potash and twenty or twenty-five of water, a portion is at once dissolved, the remainder, on the application of a gentle heat, forming a reddish yellow transparent solution, from which acids throw down a yellow precipitate, which is a mixture of two substances, one of a light lemon colour, the other brown, or almost black. The appearance of the changes which are here undergone, varies considerably with the manner of performing the operation. If, for instance, the sulphocyanogen be in excess, it is either not entirely dissolved, or if it is, the precipitate produced by acids contains unaltered sulphocyanogen: on the contrary, if the alkali be in excess, the decomposition is more complete, the precipitate has a much lighter colour, and does not appear

so dense as in the former case. The length of digestion also considerably affects the result; the longer it is continued (within certain limits) the more complete is the action: and lastly, the method of preparation of sulphocyanogen likewise influences it; while that prepared by chlorine becomes red at the commencement of the action, that prepared by nitric acid becomes yellow. The cause of this difference will be immediately explained.

Of the two substances precipitated by acids, the lemon-coloured one forms by far the largest portion; and not having obtained the other pure, I have as yet paid little attention to it, although it appears intimately connected with the decompositions which occur. The last is insoluble in water and alcohol, while the lemon-coloured substance is soluble in both these liquids, which can consequently be used to separate them.

I have found the following method of procedure to be the most convenient. Take three parts of sulphocyanogen (that prepared by nitric acid is preferable) and four of potash, or one of sulphocyanogen, and 27 or 28 parts of solution of caustic potash in common use (sp. gr. 1.06), keep this mixture at a gentle heat (120°) for about three hours, and then boil for half an hour. It is then entirely dissolved, but on cooling a small quantity of the black matter separates, which must be removed by filtration. To the filtered solution add hydrochloric or dilute sulphuric acid, which throws down the mixture in question, sometimes of a bright lemon-colour, but more frequently darker. It must be collected on a filter and washed with cold water until all the chloride of potassium or sulphate of potash is removed. Boiling alcohol must be used to purify it, as hot water dissolves too minute a portion to be conveniently employed for this purpose: the filtered alcoholic solution can be distilled nearly to dryness, which gives the substance perfectly pure in the form of a flocculent lemon-yellow crystalline powder.

Its taste is intensely bitter and acrid, but not immediately perceptible, on account of its slight solubility: it thickens the saliva, and a minute portion of its dust inhaled causes sneezing. One part requires rather more than one thousand of cold water to effect its solution. Boiling water dissolves 2.36 per cent. Cold alcohol takes up 4 per cent., boiling alcohol about 14 per cent. Wood spirit possesses about the same solvent power on it as alcohol.

When ignited in the air sulphur burns, and a brown substance remains, which is entirely dissipated by a strong red heat: heated in a tube, sulphur, bisulphuret of carbon and

sulphuretted hydrogen are given off, leaving the same brown matter. It is soluble in concentrated sulphuric acid without change, and is again precipitated on the addition of water. Nitric acid completely decomposes it, giving rise to sulphuric, carbonic and nitrous acid. Hydrochloric acid dissolves a little without change.

When its alcoholic solution is evaporated it appears to give crystals, but this is not the case. It is owing to a bright pellicle which had been formed on the surface of the alcohol contracting, and presenting this appearance. The strong alcoholic and wood-spirit solutions are precipitated by water. All its solutions are yellow; they redden litmus paper slightly, but some time is required to effect this; it would at first be said to be neutral. From its behaviour with metallic bases and the mode of its formation, it is manifestly entitled to be classed among acids; in short, it appears from my experiments, that as obtained by the above process it is a hydrate of an hydracid which is quadribasic, losing four atoms of hydrogen and acquiring four of a metal. All its salts that I have examined are coloured, being either yellow, brown, or black. They are uncrystallizable, and those which are soluble are partially decomposed by evaporation: for this reason, I have been unable to obtain any definite soluble salt in a state fit for analysis, and the insoluble salts that I have examined contain a large excess of acid. According to my experiments, its empirical formula will be $S_{12} C_{10} N_5 H_6 O_2$; its rational formula $S_{12} Cy_5, H_4 + 2 aq.$

The following are the results of my analyses.

11.01 grains of the pure substance (prepared by the above process), dried at 212° , gave 7.00 grains of carbonic acid and 1.78 grains of water, or 17.58 of carbon and 1.79 of hydrogen per cent. Other analyses have given a mean of 17.60 of carbon and 1.74 of hydrogen. The mean of three estimations of the sulphur, (by heating with nitric acid which readily decomposes it, and precipitation of the sulphuric acid by nitrate of barytes) is 55.16 per cent. The nitrogen was estimated in the usual manner, by observing the relation between it, and the carbonic acid as produced by combustion by oxide of copper; but the product was collected in one receiver instead of several small tubes. The results are as follows:

Barometer 30.2 inches.

Mixture in receiver, 85 measures over .6 inch of mercury, equal to 83.3 measures, common pressure.

After absorption of carbonic acid, there remained 32 measures, over 3.4 inches of mercury, equal to 28.3 measures, common pressure.

$83\cdot3 - 28\cdot3 = 55\cdot0$; therefore $83\cdot3$ of mixture contain $28\cdot3$ of nitrogen, and $55\cdot0$ of carbonic acid: and as $28\cdot3 : 55\cdot0 :: 1 : 1\cdot94$; or as 1 of nitrogen to 2 of carbon nearly.

We have then for the per-centage,

Sulphur	55·16
Carbon	17·59
Nitrogen	20·37
Hydrogen	1·76
Oxygen (loss) . .	5·12

100·00

which numbers closely correspond to

		Calculated.
12 Sulphur	2414·	55·64
10 Carbon	764·3	17·61
5 Nitrogen	885·2	20·42
6 Hydrogen	74·8	1·72
2 Oxygen	200·0	4·61
1 equivalent	4338·3	100·00

Since it appears to be a hydrated hydracid of a sulphuret of cyanogen, the name hydrothiocyanic acid will perhaps not be inapplicable. Its salts will then be thiocyanides, and its symbol may be Thcy H_4 .

Thiocyanides.—The alkaline reaction of potash, soda, ammonia, and barytes cannot be completely destroyed by any excess of this acid, although the pure salts are neutral. The alkaline carbonates are not decomposed by it at common temperatures, but if boiled, carbonic acid is evolved, and a salt of the alkali formed. Its solutions in potash and soda give yellow uncrystallized residues on evaporation. The acid was digested some time with ammonia, filtered and evaporated over sulphuric acid *in vacuo*: this gave a yellow uncrystallized salt, soluble in water, giving a solution neutral to test paper, and intensely bitter; but free acid had been deposited during evaporation, notwithstanding it was at first highly alkaline.

Barytes.—The acid was digested with barytes, water, and carbonic acid gas passed through the solution to separate excess of barytes: a yellow solution remained, which on evaporation gave a yellow salt, but mixed with crystals of hydrate of barytes.

A solution of the acid produces no precipitate in solutions of salts of magnesia, both oxides of iron, manganese, zinc, or nickel.

Copper.—A solution of the acid produces an ochre-brown

precipitate in solution of sulphate of copper. The precipitate is decomposed by the hydrochloric, nitric and concentrated sulphuric acids; also by sulphuretted hydrogen, the hydrothiocyanic acid being liberated. It is blackened by alkalis, an alkaline thiocyanide being produced: the black or brown substance remaining appears to be a subsalt. When the thiocyanide of copper is ignited in a tube, it gives off sulphur, hydrated cyanic acid, and bisulphuret of carbon, leaving a residue of sulphuret of copper.

Lead.—Solutions of hydrothiocyanic acid produce a yellow precipitate in acetate and subacetate of lead, which is decomposed by the stronger acids, and by sulphuretted hydrogen, the acid being reproduced unaltered in the last case. Nitric acid instantly produces sulphate of lead. On being heated in a tube, it gave rise to similar products as the copper salt; hydrated cyanic acid, sulphur, bisulphuret of carbon, and sulphuret of lead. Like most insoluble salts of slightly soluble acids (when prepared from solutions of the acid), this is contaminated with a large excess of acid. Two combustions by oxide of copper gave

1. 8.62 per cent. of carbon	•49 of hydrogen.
2. 8.72	•50

For the lead: 1. 12.3 grains treated with nitric acid gave 9.36 grains of sulphate of lead, equal to 51.96 per cent. of lead.

2. 10.77 treated in the same manner, gave 8.19 of sulphate, or 51.93 per cent.

Calculated according to the formula $\text{Thcy } 4 \text{ Pb} + 4 \text{ aq}$, it should contain 7.90 of carbon, .51 of hydrogen, and 53.40 of lead per cent.; notwithstanding this difference, no other formula could have been selected, in accordance with the ultimate composition obtained for the acid.

Like the copper salt, thiocyanide of lead is blackened by alkalis, a subsalt being produced.

Silver.—The behaviour of the solution of hydrothiocyanic acid with nitrate of silver is very peculiar and characteristic, forming a test sufficiently delicate to detect one part of the acid in 10,000 of water. On mixing the solutions a yellow flocculent precipitate is formed, which on standing a short time, or immediately on heating, aggregates, changing to a black colour, without any evolution of gas or odour of cyanogen. The change is not hastened by solar light. When the black substance is treated with sulphuretted hydrogen, the acid is reproduced, and sulphuret of silver formed. Concentrated sulphuric acid has no effect on it; if diluted, sulphate of silver is formed, and the acid is liberated. It is soluble

with decomposition in nitric acid, and by boiling in hydrochloric acid, sulphuretted hydrogen being evolved in the latter case. The affinity of silver for the radical of this acid appears stronger than for chlorine; for if this acid and hydrochloric acid be present in the same solution, and nitrate of silver added, the thiocyanide of silver is first precipitated. It is insoluble in ammonia.

19·60 grains were treated with hot nitric acid, which dissolved it; the silver was precipitated by hydrochloric acid, and nitrate of barytes added to the filtered solution to obtain the sulphuric acid.

The chloride of silver amounted to 18·25 grains, or 13·747 of silver, equal to 70·14 per cent. The sulphate of baryta amounted to 21·24 grains, equal to 2·94 of sulphur: sulphur not converted into sulphuric acid, separated on a weighed filter, amounted to ·20 grain, = 3·14 or 16·01 per cent. of sulphur.

The proportion of silver here is just twice as great as the proportion of lead in the salt of that metal. In fact, it appears to be a double thiocyanide and oxide of silver, or a subsalt: thus $They\ 4\ Aq + 4\ Aq\ O$, which by theory should contain

		Found.
Sulphur	15·80	16·01
Silver	70·78	70·14

As the neutral thiocyanides are yellow and the subsalts black, so we have reason to believe that the yellow silver compound is the neutral thiocyanide; but this cannot be decided by analysis, on account of the rapid change which it undergoes.

Mercury.—Nitrate of protoxide and chloride of mercury are precipitated by the aqueous solution of this acid. The precipitate is at first white, but by heating it becomes yellow. In its properties it resembles the copper and lead salts; like them it is converted into a subsalt by alkalies, and it gives similar products on heating. Nitric acid instantly acts on it; a white compound is produced which undergoes no further change by nitric acid alone, but on the addition of hydrochloric acid it is immediately dissolved.

Nitrate of suboxide of mercury gives the black subthiocyanide. Chloride of platinum and bichloride of tin are precipitated yellow by solutions of the acid, but I have not examined these precipitates.

Such is the incomplete investigation I have had it in my power to make of these compounds. As the sulphocyanogen derived from the sulphocyanides does not appear to be what it was considered, but a highly complicated substance, the in-

terest with which the products of its decomposition by alkalis will be viewed, is materially lessened. For this reason I have not pursued this subject so completely as I intended, nor, perhaps, as it deserves. Although I have been led by a point in this investigation, which I could not comprehend, to the discovery that sulphocyanogen, if not exactly so composed as I have represented it, is certainly not the radical of the sulphocyanides, but a product of its decomposition; yet the point in question has not been ascertained, namely, the changes that sulphocyanogen undergoes by alkalis.

At first I imagined the black substance which separated from the solution of sulphocyanogen in alkali, on cooling, and the brown or black substance precipitated by acids, *insoluble in water and alcohol*, as accidental or merely secondary products: I now look on these differently. When the last brown substance is digested again in an alkali, more hydrothiocyanic acid is separated; the remainder is quite black, and is evidently identical with the first black substance. It appears more to resemble paracyanogen than anything else with which I am acquainted; but not having obtained it pure, and but in small quantity, I have not been able to decide this point. Sulphocyanide of potassium, and sulphite of potash, are likewise formed when sulphocyanogen is digested in potash, but the sulphite is in very small quantity, and I believe accidental.

Hydrothiocyanic acid is not produced by potash and soda only; barytes, ammonia, and even the alkaline carbonates give birth to it. Its production by ammonia is remarkable. The sulphocyanogen becomes light yellow, but is not dissolved; sulphocyanide of ammonium, with only a trace of thiocyanide, is formed. The yellow substance into which the sulphocyanogen is converted is, however, hydrothiocyanic acid.

This proves that whatever may be the action of the alkali, it is not the affinity of its radical for the radical of the acid (or as it may be called, thiocyanogen) which is the leading cause; for if so, the whole of the acid should have existed as thiocyanide of ammonium.

It is also probable that the action is an oxidizing one, for I find that the acid is produced by the action of chlorine and nitric acid on sulphocyanogen. Indeed, it is difficult to prepare sulphocyanogen by nitric acid without its production, and hence I have recommended that prepared by this means as preferable to that by chlorine for the preparation of this acid*. It has been observed, that when chlorine is passed into

* If a mixture of sulphocyanogen and hydrothiocyanic acid be digested

a dilute solution of sulphocyanide of potassium, the sulphocyanogen is precipitated of a light yellow colour, and does not subside readily. I have ascertained, by experiment, that this light yellow substance is not sulphocyanogen, but hydrothiocyanic acid. It is, however, more difficult to convert sulphocyanogen into this acid by chlorine, than by nitric acid; chlorine consequently gives the purer substance.

In conclusion, I would state, that these experiments have been performed in Prof. Graham's laboratory, to whom and to his late assistant Mr. Fownes, I am happy in acknowledging myself indebted for their suggestions during this research.

University College, June 25, 1840.

XXXVIII. *On the Use of Hydriodic Salts as Photographic Agents.* By Mr. ROBERT HUNT.

[Continued from p. 211, and concluded.]

38. *On the darkening of the Photograph.*

MR. TALBOT first directed attention, at the last meeting of the British Association, to a peculiarity possessed by some of these kinds of photographs, namely, that they were neither fixed nor otherwise; but that on exposure to sunshine they changed in their dark parts from a red to a black, the lights of the picture being unaffected by the light.

39. This singular effect I have proved to be entirely dependent on the influence exerted by the less refrangible rays of the solar spectrum in exalting the oxidation of the silver; but a brief statement of some effects produced by the dissevered rays, will place the matter in a much clearer light.

40. By allowing a very intense prismatic spectrum, formed by a flint-glass prism, to fall upon any of these photographs which blacken by white light, it will be found that the darkening process commences in the red ray, at which point it goes on with the greatest intensity, and is gradually shaded off to the lowest edge of the extreme red; the shading is also continued through the orange and yellow rays being sharply cut off at that line of the spectrum where the pure green is visible.

41. As it was not possible to pursue my inquiry on the effects of the spectrum with any degree of satisfaction without a heliostat, an instrument I have not the means of procuring,

in alkali, the sulphocyanogen is first dissolved, leaving the acid. This explains why sulphocyanogen prepared by nitric acid becomes yellow when treated with alkali, which is not the case with that prepared by chlorine.

I turned my attention to the effects produced by the light which had permeated coloured media, the absorptive powers of which were carefully analysed.

The media I was induced to adopt transmitted rays in the following order.

BLUE. *Ammonia-Sulphate of Copper.*—The whole of the most refrangible rays, from the edge of the green to the extremity of the violet.

GREEN. *Nitro-muriate of Copper.*—Those rays which have place between the extreme upper edge of the blue, and a line which would accurately divide the pure yellow.

YELLOW. *Bi-chromate of Potassa.*—That portion of the spectrum which would lie between a line drawn below the orange, rather within the red ray, and through the lower edge of the pure green ray.

RED. *A strong Solution of Carmine in Ammonia.*—A portion of the orange and all the rays below it.

42. The most remarkable effects were produced upon the papers *a, b, c, d,* and *n* (13). They have been subjected to similar influences, prepared with all the hydriodates I have mentioned (20—27); but I do not feel myself warranted in occupying your pages with any statement of the results on any, but those prepared with the pure hydriodate of iron and the hydriodate of baryta. These drawings were all well washed with hot water, and when quite dry, arranged under the different fluids, and exposed in a window which faces the south. I will name the papers from the salt used, and the colour shall indicate the rays.

43. *Hydriodate of Iron, Muriate of Ammonia.*—**BLUE.** The picture nearly destroyed by the browning of the yellow lights, at the same time as the darker parts have much faded.

GREEN. The dark parts nearly all faded out; the few remaining spots much reddened, but no change in the yellow of the light parts.

YELLOW. Looking through the paper, the lights appear darkened by a blueish-green tinge; the dark parts, originally a red brown, are changed to a blue-black.

RED. The lights yellower than before; the darks a deep black.

44. *Chloride of Sodium.*—**BLUE.** The lights darkened, and the dark parts faded and reddened. **GREEN.** Picture entirely obliterated; the yellow unchanged. **YELLOW.** The lights tinged a decided blue; shadows darkened.

RED. The lights of a green tinge; but I consider this to arise from the deepening of the yellow hue; the dark parts blackened.

45. *Muriate of Strontia*.—BLUE. These are more permanent than any other variety of the hydriodic photographs. Under this influence the shadows are browner than before; the lights scarcely changed. GREEN. The yellow much increased in depth; the dark parts faded slightly and become very red. YELLOW. The lights very little tinged with blue; the darks without any apparent change. RED. The lights deepened; the shadows a fine black.

46. *Muriate of Baryta*.—BLUE. The yellow parts are become brown; the dark portions faded and reddened. GREEN. Lights unchanged; the dark parts very red. YELLOW. Lights unchanged; shadows tinged green, over a very decided blackening which has taken place. RED. The yellow much heightened; the dark parts much tinged with green.

47. *Hydrochloric Acid*.—BLUE. Faded out: lights darkened. GREEN. Faded out; yellow much increased. YELLOW. The lights rendered very yellow; darks unchanged. RED. Yellow become very strong; the shadows are very much blackened.

48. *Hydriodate of Baryta*.—Under this head it will only be necessary to name the effects on three kinds of photographs, the others being very similar in all their changes to those just mentioned.

49. *Muriate of Ammonia*.—In my paper on the influence of coloured media, vol. xvi. p. 270 of your Journal, I have already mentioned the singular change which ensues upon exposing this kind of drawing to light under media such as we are now considering. To that paper I refer you.

50. *Muriate of Baryta*.—BLUE. Faded in the dark parts, which are become a brick red; the yellowness of the lights increased. GREEN. The lights unchanged; the shadows suffused with a pink hue. YELLOW. Lights unchanged; shadows much darkened and strongly tinged with a light blue. RED. Lights unchanged; dark parts a deep blue. These singular effects, which, although they are traceable on nearly all those photographs which blacken by after exposure to sunshine, are much more decided when the salts of baryta in one or other of the processes have been used. I communicated these facts with others to Sir John Herschel, who has paid me a very high compliment by inserting my communication in his valuable memoir "On the Chemical Action of the Solar Spectrum." I the less regret my inability to pursue my observations on the effects of the pure prismatic rays on the hydriodic preparations, finding that the subject is one which, among others equally curious and important, is engaging the attention of this eminent philosopher.

51. *Muriate of Strontia*.—BLUE. Lights but very little

changed; darks faded and reddened. GREEN. Lights unchanged; shadows less faded, not so red. YELLOW. Lights unchanged; dark parts a blue-black. RED. Lights unchanged; dark parts become very black.

52. From a careful perusal of these results it will appear that this curious darkening of the finished picture is most evident under the influence of red light, but that this property extends up to the green rays, beyond which a different power is exercised; the *deoxidizing* influence appearing to be greatest in the blue rays, whilst the yellow iodide of silver suffers decomposition in the most refrangible rays.

53. *The fading of Hydriodic Photographs.*—I have before noticed (30.) the want of absolute permanence in these pictures. The study of the *modus operandi* of solar light in its action on them opens some very remarkable facts in relation to the iodide of silver, which when first observed, led me to believe the existence of two distinct salts, whereas I now entertain a different opinion. The drawing fades first in the dark parts, and as they are perceived to lose their definedness, the lights are seen to darken, until at last the contrast between light and shadow is very weak.

54. If a dark paper is washed with an hydriodate and exposed to sunshine, it is first bleached, becoming yellow; then the light again darkens it; if, when quite dry, it is put away in the dark, it will be found in a few days to be again restored to its original yellow, which may be again darkened, but not so easily as at first, and the yellow colour is again restored in the dark. The sensitiveness to the influence of light diminishes after each exposure, but I have not been enabled to arrive at the point at which this entirely ceases.

55. If a dark paper, bleached by an hydriodate and light, be again darkened, and then placed in a bottle of water, the yellow is much more quickly restored, and bubbles of gas will escape freely, which examination will show to be oxygen.

56. By inclosing pieces of hydriodated paper in a tube to darken, we discover, as might have been expected, some hydrogen is given off. If the paper is then well dried and carefully shut up in a warm dry tube, it remains dark; moisten the tube or the paper, and the yellowness is speedily restored.

57. Take a photograph thus formed and place it in a vessel of water, in a *few days* it will fade out, and bubbles of oxygen will accumulate around the side. If the water is examined, there will be found no trace of either silver or iodine; thus it is evident the action has been confined to the paper.

58. We see that the iodide of silver has the power of sepa-

rating hydrogen from its combinations. I cannot regard this singular salt of silver as a definite compound: it appears to me to combine with iodine in uncertain proportions. In the process of darkening the liberation of hydrogen is certain; but I have not in any one instance been enabled to detect free iodine; of course it must exist either in the darkened surface, or in combination with the unaffected under layer; possibly this may be the iodide of silver, with iodine in simple mixture, which, when light acts no longer on the preparation, is liberated, combines with the hydrogen of that portion of moisture which the hygrometric nature of paper is sure to furnish, and as an hydriodate again attacks the darkened surface, restoring thus the iodide of silver. This is strikingly illustrative of the fading of the photograph. The picture is light iodide of silver and dark oxide of silver; as the yellow salt darkens under the influence of light it parts with its iodine, which immediately attacks the dark oxide, which is gradually converted into an iodide, oxygen, as I have shown, being liberated. The following experiments go not only to prove this position, but also serve to illustrate in some measure the action of light on this compound.

59. *Iodide of Silver*.—Precipitate with any hydriodate, silver from its nitrate in solution, and expose the vessel containing it, liquid and all, to sunshine, the exposed surfaces of the iodide will blacken; remove the vessel into the dark, and after a few hours all the blackness will disappear: we may thus continually restore and remove the blackness at pleasure.

60. If we well wash and then dry the precipitate it blackens with difficulty, and if kept perfectly dry it continues dark; but moisten it and the yellow is restored after a little time.

61. In a watch-glass, or any capsule, place a little solution of silver; in another, some solution of any hydriodic salt; connect the two with a filament of cotton, and make up an electric circuit with a piece of platina wire, expose this little arrangement to the light, and in a very short time it will be seen that iodine is liberated in one vessel, and the yellow iodide of silver formed in the other, which blackens as quickly as it is formed.

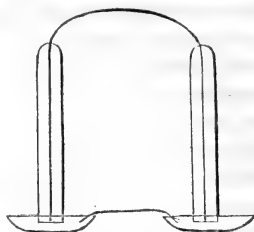
62. Place a similar arrangement to the above (61.) in the dark, iodine is slowly liberated. *No iodide of silver formed*, but around the wire a beautiful crystallization of metallic silver.

63. A piece of platina wire was sealed into two glass tubes; these when filled, the one with hydriodate of potassa in solution, and the other with a solution of the nitrate of silver, were reversed into two watch-glasses containing the

same solutions, the glasses being connected with a piece of cotton, as in the adjoining figure.

A few hours of daylight occasioned the hydriodic solution in the tube to become quite brown with liberated iodine; a small portion of the iodide of silver was formed along the cotton, and at the end dipping in the salt of silver. (The glasses were kept purposely wide apart to prevent a quick formation.)

During the night the hydriodic liquid became again colourless and transparent, and the dark salt along the cotton was as yellow as at first.



64. A curious illustration of the action I have endeavoured to elucidate, may be had by operating with chlorine. Its first action on one of these hydriodic photographs is the separation of iodine, which, when exposed to the light, is seen to act on the edges first, and gradually over the whole extent of the darkened portions. This curious action may be repeated until all the iodine is removed from the paper.

I think it will now be evident, that before we can expect to have quite permanent and well-finished hydriodic photographs, we must have at command the means of removing all the iodide of silver without injuring the dark oxide.

65. *On the Action of the dissevered Rays of the Solar Spectrum on dark photographic Papers washed with an hydriodate Liquid.*—Sir John Herschel, in his valuable memoir before-mentioned, has clearly shown, “that the total effect of a ray of white light on iodic preparations, is in fact the difference of two opposing actions, either of which may be exalted or enfeebled at pleasure by circumstances under our command, but difficult to reproduce exactly at our pleasure. When these opposing actions,” I still quote Sir John’s words, “exactly neutralize each other, the paper is insensible. When either preponderate, it is positive or negative in its character, according to that of the preponderant action; nay, it may at one and the same moment be positive to light incident under certain circumstances, insensible under others, and negative under a yet different illumination.”

66. These singular facts were noticed by me in a very early stage of my inquiries; and you may perhaps remember my forwarding to you, with some specimens illustrative of my paper on “The Chemical Action of the Solar Spectrum,” an hydriodated photograph, which exhibited the effects of coloured media in determining the action*.

* See Sir John Herschel’s memoir “On the Chemical Action of the Rays of the Solar Spectrum,” Phil. Trans. 1840, Part i. page 43.

67. It is essential to a right understanding of the following results, that the absorptive power of the media I used in my experiments be set forth. Into a frame were fixed four coloured glasses.

A PURPLE GLASS, cutting off all the rays below the green, a portion of the green being also absorbed.

A GREEN GLASS, admitting the permeation of those rays only which lie between the least refracted extremity of the blue and the extreme orange. A portion of the yellow rays are absorbed.

A PALE AMBER GLASS, shortening the spectrum by the violet and indigo rays only.

A RED GLASS, absorbing all the most refrangible rays, permitting those only which lie below the blue to permeate.

68. If any of the sensitive darkened photographic papers (*a, b, c, d, o,*), washed with a good hydriodic solution, be placed in close contact, face to face, with an engraving which has been rendered transparent by being well soaked in water, and exposed to sunshine with the above frame superposed, we produce (to use Sir J. Herschel's nomenclature) a positive and a negative photograph on the same sheet. Beneath the blue glass the picture is copied as perfectly, but not quite so quickly, as under a colourless glass, the lights of the engraving being correctly copied on the photograph.

Beneath the green glass the lights and shadows of the photograph are completely reversed. In all the parts which correspond with the lights of the engraving the oxidation is much exalted, the paper having assumed a defined blackness. The darker parts of the engraving are copied in lights not simply formed by the contrast of the original brown of the paper with the induced blackness, but by a positive brightening of the parts.

Beneath the yellow glass the results are singularly uncertain. Often on the same sheet, with the same hydriodic solution, two experiments will give totally different results. I send you two specimens in proof of this, prepared in every way alike, and both executed within the same half-hour.

Under the red glass a reversed picture is formed, *not by the darkening of the oxide*, which retains its original colour, but by the eating out of strong lights under the dark parts of the engraving.

69. From these results it is evident that the blackening action on the wet hydriodidated paper is dependent on a different class of rays from those which blacken the finished drawings: in the wet process I ever find the maximum of darkness beneath the green glass, or rather within the limits of the green and yellow rays, little or no darkening effect being evident in the red rays; whereas on the dry picture

the maximum of effect is in the red rays (42—50.). I am inclined, however, to believe, that examination of these phenomena by a fixed spectrum will prove a shifting of the actions according to the kinds of paper used; but I am satisfied the entire action will be confined in the one instance to the green and yellow rays, and in the other to the orange and red rays.

70. The very curious brightening of those parts of the photograph corresponding with the darks of the engraving, attracted my attention powerfully. My first impression was, that the carbonaceous matter of the ink used in printing exerted a kind of catalytic action in directing the formation of an iodide of silver. I think I have proof of this to some extent.

71. Most printed pages, unless they are very old, when placed in juxtaposition *in the dark* with an hydriodidated photographic paper, leave an impression after some hours, and I have succeeded in partially copying some engravings thus. However, the result is uncertain; the copy, at all times faint, is often very imperfect, being sometimes bleached in circles, of which a letter or two form the centres; at other times the letters are copied, but all of them shaded from an extension of the bleaching action.

72. From effects I noticed from the accidental contact of some carbonate of iron, I was sanguine of being enabled to copy a written page. In this I was disappointed. I have not, with any of the numerous kinds of writing ink which I have tried, succeeded in obtaining the slightest trace of a letter on the photographic paper.

73. From the rapidity with which this effect is produced when the photograph and engraving are exposed to light, it is evident some other cause than the one I have just considered was in active operation. A careful examination of the photographs formed under the before-mentioned glasses, particularly under the red glass, convinced me that the quickening agent was to be sought for in the calorific rays, which are absorbed and retained with greater force by the dark parts of the engraving.

74. To put this notion to the test, I placed a printed page in contact with a paper wetted with an iodidated solution, over which I placed a glass, and then a plate of copper, which I made hot by rubbing it with a heated iron. The passage of the heat through the glass was sufficient to effect as fair a copy as is produced under the red glass by the influence of light.

75. These researches, which were pursued with a view

of the two crystals, the observed values of $p' p'''$ were $65^\circ 34' 24'' - 36'' - 51'' - 27''$; those of $p p''$ were $65^\circ 34' 27'' - 53'' - 25'' - 35''$. The values of $p' p'''$, $p p''$ given by the second crystal, agree very well with each other, and with the larger of the two values of $p p''$ given by the first. The mean of these three, $65^\circ 34' 32''$, will probably be very near the truth.

The sphere of projection (fig. 2.) exhibits the poles of all the faces observed by Mohs and Levy, as well as by myself. The symbols of the simple forms are

g	$\{110\}$,	h	$\{210\}$,	l	$\{100\}$,
r	$\{320\}$,	x	$\{410\}$,	c	$\{001\}$,
p	$\{101\}$,	s	$\{111\}$,	e	$\{310\}$,
z	$\{321\}$,	t	$\{313\}$,	u	$\{710\}$.

The calculated angles between normals to the faces are

$l l'$	$90^\circ 0'$	$l r$	$33^\circ 41'$	$p p'$	$45^\circ 2'$
$l g$	$45 \quad 0$	$c l$	$90 \quad 0$	$s s'$	$56 \quad 52$
$l u$	$8 \quad 7.8$	$c p$	$32 \quad 47.3$	$p t$	$10 \quad 14$
$l x$	$14 \quad 2$	$c s$	$42 \quad 20$	$z z'$	$20 \quad 46$
$l e$	$18 \quad 26$	$c t$	$34 \quad 10.6$	$z z''$	$61 \quad 14.8$
$l h$	$26 \quad 33.9$	$c z$	$66 \quad 42.3$	$p z$	$41 \quad 43.3$

z is the intersection of the zone circles $p p'''$, $s p''$, $c r$; t is the intersection of the zone circles $p l'$, $c e$.

Of the crystals above-mentioned, one is a combination of the simple forms having the faces l, g, h, e, p, s ; the other of those having the faces g, h, e, u, p, s ; t was observed by Levy (*Déscription d'une Collection de Minéraux*).

In the collection of minerals presented to the University by Professor Whewell, a crystal occurs which is a combination of the forms having the faces h, c, r, p, z , and also others which are combinations of the forms having the faces x, p , and occasionally c, l, g, h, e . Among the latter are several twins (fig. 3.), in which the faces $c p, c_i p$, are all in one zone, and the angle between normals to $c c_i$, rather more than 55° . On account of the unevenness of the faces, this angle could not be accurately measured; if the twin plane were parallel to a plane v , the pole of which is the intersection of the zone circles $z z''$, $c l$, the symbol of v would be (301) , $c v = 62^\circ 38' 4$, $c c_i = 54^\circ 43' 2$. Hence probably the twin plane is parallel to a face of the form $\{301\}$. The twins of most usual occurrence are those described by Haidinger (*Edinburgh Journal of Science*, vol. iii. p. 62.). Mr. Brooke measured the angle between the faces $l l_i$ (fig. 4.) of one of these twins in his collection, and found that it agreed perfectly with the supposition that the twin plane was parallel to one of the faces p .

The values of cp , cs , deduced from the observed values of pl , sg , given by W. Phillips in his *Mineralogy* (third edition), are $32^{\circ} 45'$ and $42^{\circ} 20'$. My determination of these angles differs only about two minutes from the former, and agrees

Fig. 3.

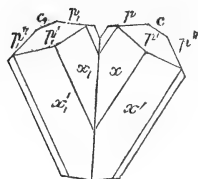
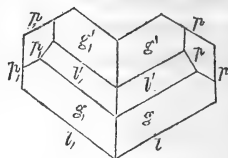


Fig. 4.



exactly with the latter. Unfortunately the figures which were intended to express the angles pp'' , ss'' ("a on a over summit = 90° , c on c over summit = $109^{\circ} 47'$ ") are very erroneous, probably through some mistake in transcribing them, for the errors (which remain uncorrected in the fourth edition) are much too large to have been errors of observation.

St. John's College, Aug. 31, 1840.

W. H. MILLER.

XL. *Note upon Mr. Griffith's Paper "upon the true Order of Succession of the Older Stratified Rocks near Killarney and Dublin."* By CHARLES WILLIAM HAMILTON, F.G.S. &c.*

THERE are a few points in this paper to which I think it necessary to reply.

I brought forward the result of my own observations with diffidence, because I felt that they had been few and imperfect; but I think they are not useless, and that I have, as I then hoped, "pointed out an interesting field of inquiry, and helped to show that a large portion of Ireland is still a subject for Geological debate;" and besides, I am still convinced that my observations, as far as they went, were not inaccurate, as Mr. Griffith asserts, although his subsequent observations may have shown that some of my conclusions were so.

Mr. Griffith has represented the fault which is observable at Brickeen Island (*L. & E. Phil. Mag.* vol. xvi. Pl. 2. fig. 2.) as involving an upcast of about *three thousand feet*, and the beds of chloritic slate, quartz-rock, and old red sandstone as cropping out at a low angle on the south-eastern slopes of Glenna mountain, at the base of which the fault is to be seen.

Now in this case I am thoroughly convinced that my re-

* Communicated by the Author.

presentation is correct and Mr. Griffith's incorrect; and as it is a point easily accessible to any geologist who may visit Killarney, I do hope that some one will take the trouble of comparing the two observations. I believe that the succession of the strata is in exactly the reversed order, that there is an anticlinal on the summit of Glenna mountain, that the strata on the south-eastern slopes dip to the S.E. at an angle approaching very nearly to the perpendicular, and that consequently the slates at the back of Lady Kenmare's cottage are in the *highest* and not the *lowest* part of the series there developed.

Mr. Griffith attaches much importance to the difference of strike upon the opposite sides of the fault. I have made a great many careful observations, and although until we get a correct map it would be impossible to lay these down so as to draw any correct conclusions from them, I may observe that the observations taken at the same side of the fault vary quite as much among themselves as they do with those on the other side.

In p. 173 Mr. Griffith says, "it appears to me that Mr. Hamilton is mistaken in separating the old red sandstone from the Devonian system;" but the very quotation he makes explains clearly that I made no such separation, but referred the compact arenaceous rocks overlying the coarse red conglomerate to the *upper part* of the Devonian system.

I shall not occupy your pages with other points in which I am still hardy enough to rely upon my own observations in opposition to so high an authority as my friend Mr. Griffith; but I could not leave these unnoticed, because I wish to press upon the attention of geologists the fact that the difficulties of this district have not yet been cleared away, and that Mr. Griffith is premature in referring to the Silurian epoch, that vast depth of sandstone and conglomerate which occurs between the bays of Kenmare and Castlemaine, and the whole section between Foillatarriv and Brandon; I have before expressed my opinion (Journal of the Geological Society of Dublin, vol. i. p. 282.) that this latter section bears the strictest analogy to that between the Bangor quarries and Llyn Schal, and I see no reason for retracting that opinion.

To Mr. Weaver's remarks and insinuations of "an unrestrained indulgence of fancy," &c., I shall not reply; his objections to the possibility of the old red sandstone dipping in one place to the south, and another to the north, seem hardly to require an answer; and the correctness of his observations has been already subjected to a sufficiently rigorous inquiry by Mr. Griffith,

XLI. On the Heat of Vapours and on Astronomical Refractions. By JOHN WILLIAM LUBBOCK, Esq., Treas. R.S. F.R.A.S. and F.L.S., Vice-Chancellor of the University of London, &c.

[Continued from vol. xvi. p. 569.]

On the Vapours of Æther, Alcohol, Petroleum, and Oil of Turpentine.

THE following Table is extracted from a valuable paper by Dr. Ure in the Phil. Trans. for 1818.*

Table of the pressure of the vapours of æther, alcohol, petroleum or naphtha, and oil of turpentine.

Æther.		Alcohol sp. gr. 0·813.		Alcohol sp. gr. 0·813.		Petroleum.	
Temp. °	Pressure.	Temp. °	Pressure.	Temp. °	Pressure.	Temp. °	Pressure.
Fahr.	Inch.	Fahr.	Inch.	Fahr.	Inch.	Fahr.	Inch.
34	6·20	32	0·40	193·3	46·60	316	30·00
44	8·10	40	0·56	196·3	50·10	320	31·70
54	10·30	45	0·70	200	53·00	325	34·00
64	13·00	50	0·86	206	60·10	330	36·40
74	16·10	55	1·00	210	65·00	335	38·90
84	20·00	60	1·23	214	69·30	340	41·60
94	24·70	65	1·49	216	72·20	345	44·10
104	30·00	70	1·76	220	78·50	350	46·86
		75	2·10	225	87·50	355	50·20
		80	2·45	230	94·10	360	53·30
2nd.	Æther.	85	2·93	232	97·10	365	56·90
105	30·00	90	3·40	236	103·60	370	60·70
110	32·54	95	3·90	238	106·90	372	61·90
115	35·90	100	4·50	240	111·24	375	64·00
120	39·47	105	5·20	244	118·20	Oil of Turpentine.	
125	43·24	110	6·00	247	122·10	Temp.	Pressure.
130	47·14	115	7·10	248	126·10	304	30·00
135	51·90	120	8·10	249·7	131·40	307·6	32·60
140	56·90	125	9·25	250	132·30	310	33·50
145	62·10	130	10·60	252	138·60	315	35·20
150	67·60	135	12·15	254·3	143·70	320	37·06
155	73·60	140	13·90	258·6	151·60	322	37·80
160	80·30	145	15·95	260	155·20	326	40·20
165	86·40	150	18·00	262	161·40	330	42·10
170	92·80	155	20·30	264	166·10	336	45·00
175	99·10	160	22·60			340	47·30
180	108·30	165	25·40			343	49·40
185	116·10	170	28·30			347	51·70
190	124·80	173	30·00			350	53·80
195	133·70	178·3	33·50			354	56·60
200	142·80	180	34·73			357	58·70
205	151·30	182·3	36·40			360	60·60
210	166·00	185·3	39·90			362	62·40
		190	43·20				

[* Or Phil. Mag., First Series, vol. liii. p. 95.]

From these observations I take the following data for æther:—

$$p = 1 \qquad \theta = 73^{\circ}$$

$$p' = \frac{99.10}{30} \qquad \theta' = 143^{\circ}$$

$$p'' = \frac{166.00}{30} \qquad \theta'' = 178^{\circ}$$

$$\frac{p''^{\beta} - 1}{p'^{\beta} - 1} = [0.1523534].$$

Hence for æther

$$\beta = -.03153 \qquad \gamma = 1.0325 \qquad E = .67086.$$

For alcohol

$$p = 1 \qquad \theta = 141.0^{\circ}$$

$$p' = \frac{97.10}{30} \qquad \theta' = 200.0^{\circ}$$

$$p'' = \frac{166.10}{30} \qquad \theta'' = 232.0^{\circ}$$

$$\frac{p''^{\beta} - 1}{p'^{\beta} - 1} = [0.1830354].$$

$$\beta = .04025 \qquad \gamma = .96131 \qquad E = 1.55796.$$

For petroleum

$$p = 1 \qquad \theta = 284^{\circ}$$

$$p' = \frac{46.86}{30} \qquad \theta' = 318^{\circ}$$

$$p'' = \frac{64}{30} \qquad \theta'' = 343^{\circ}$$

$$\frac{p''^{\beta} - 1}{p'^{\beta} - 1} = [0.2259762].$$

$$\beta = -.06268 \qquad \gamma = 1.0668 \qquad E = .35294.$$

For oil of turpentine

$$p = 1 \quad \theta = 272^{\circ}$$

$$p' = \frac{45}{30} \quad \theta' = 304^{\circ}$$

$$p'' = \frac{62.40}{30} \quad \theta'' = 330^{\circ}$$

$$\frac{p''_{\beta} - 1}{p'_{\beta} - 1} = [0.2441091].$$

$$\beta = -.1816$$

$$\gamma = 1.2219$$

$$E = -.73937.$$

And hence for the vapour of æther

$$\tau = \frac{[2.2601058]}{p^{-.03153} - .67086} - 448^{\circ}.$$

For the vapour of alcohol

$$\tau = \frac{[2.5396942]}{p^{.04025} - 1.57796} - 448^{\circ}.$$

For the vapour of petroleum

$$\tau = \frac{[2.6940380]}{p^{-.06268} - .35294} - 448^{\circ}.$$

For the vapour of oil of turpentine

$$\tau = \frac{[3.1166099]}{p^{-.1816} + .73937} - 448^{\circ}.$$

The temperature being reckoned in Fahrenheit's scale and the pressure in atmospheres.

Mr. E. Russell has calculated for me the following table, showing how far the above formulæ represent the observations of Dr. Ure. The results are exhibited in the plate annexed, and it will be seen that the discrepancies between the theory here suggested and the results of observations are chiefly owing to the irregularities of the latter, which arise doubtless from the great difficulties incidental to such experiments. When the pressures are small, the variation of temperature becomes great for a small variation of pressure, so that the agreement of theory with observation may be considered as complete, even if the absolute amount of the error of the calculated temperature is then more considerable.

Æther.			Alcohol.			Sp. gr. 0·813.			Petroleum.		
Pressure.	Temp. calc. τ	Error.	Pressure.	Temp. calc. τ	Error.	Pressure.	Temp. calc. τ	Error.	Pressure.	Temp. calc. τ	Error.
Inch.			Inch.			Inch.			Inch.		
6·20	30·8	-3·2	0·40	33·6	+1·6	46·60	193·6	+0·3	30·00*	316·0	0·0
8·10	42·2	-1·8	0·56	42·8	+2·8	50·10	197·1	+0·8	31·70	320·1	+0·1
0·30	52·9	-1·1	0·70	48·1	+3·1	53·00	199·9	-0·1	34·00	325·4	+0·4
3·00	63·5	-0·5	0·86	53·3	+3·3	60·10	206·3	+0·3	36·40	330·7	+0·7
6·10	73·6	-0·4	1·00	57·2	+2·2	65·00	210·3	+0·3	38·90	335·5	+0·5
0·00	84·2	+0·2	1·23	62·6	+2·6	69·30	213·7	-0·3	41·60	340·7	+0·7
4·70	94·9	+0·9	1·49	67·8	+2·8	72·20	215·8	-0·2	44·10	345·3	+0·3
0·00*	105·0	0·0	1·76	72·4	+2·4	78·50	220·3	+0·3	46·86*	350·0	0·0
2·54	109·3	-0·7	2·10	77·4	+2·4	87·50	226·2	+1·2	50·20	355·4	+0·4
5·90	114·7	-0·3	2·45	81·9	+1·9	94·10	230·2	+0·2	53·30	360·2	+0·2
9·47	119·9	-0·1	2·93	87·3	+2·3	97·10*	232·0	0·0	56·90	365·4	+0·4
3·24	125·0	0·0	3·40	91·8	+1·8	103·60	235·6	-0·4	60·70	370·7	+0·7
7·14	129·8	-0·2	3·90	96·1	+1·1	106·90	237·4	-0·6	61·90	372·3	+0·3
1·90	135·4	+0·4	4·50	100·7	+0·7	111·24	239·8	-0·2	64·00*	375·0	0·0
6·90	140·8	+0·8	5·20	105·4	+0·4	118·20	243·3	-0·7	Oil of Turpentine.		
2·10	145·9	+0·9	6·00	110·2	+0·2	122·10	245·2	-1·8	Pressure.	Temp. calc. τ	Error.
7·60	151·0	+1·0	7·10	116·0	+1·0	126·10	247·1	-0·9	Inch.		
3·60	156·2	+1·2	8·10	120·7	+0·7	131·40	249·6	-0·1	30·00*	304·0	0·0
0·30	161·6	+1·6	9·25	125·5	+0·5	132·30	250·0	0·0	32·60	310·5	+2·9
6·40	166·2	+1·2	10·60	130·5	+0·5	138·60	252·8	+0·8	33·50	312·7	+2·7
2·80*	170·8	+0·8	12·15	135·6	+0·6	143·70	255·9	+1·6	35·20	316·6	+1·6
9·10	175·0	0·0	13·90	140·8	+0·8	151·60	258·3	-0·3	37·06	320·6	+0·6
8·30	180·8	+0·8	15·95	146·3	+1·3	155·20	259·8	-0·2	37·80	322·2	+0·2
6·10	185·4	+0·4	18·00	151·1	+1·1	161·40	262·2	+0·2	40·20	327·1	+1·1
4·80	190·2	+0·2	20·30	156·1	+1·1	166·10*	264·0	0·0	42·10	330·8	+0·8
3·70	194·9	-0·1	22·60	160·7	+0·7				45·00*	336·0	0·0
2·80	199·5	-0·5	25·40	165·7	+0·7				47·30	340·0	0·0
1·30	203·5	-1·5	28·30	170·4	+0·4				49·40	343·4	+0·4
6·00*	210·0	0·0	30·00*	173·0	0·0				51·70	347·0	0·0
			33·50	178·0	-0·3				53·80	350·2	+0·2
			34·73	179·6	-0·4				56·60	354·3	+0·3
			36·40	181·8	-0·5				58·70	357·1	+0·1
			39·90	186·1	+0·8				60·80	359·9	-0·1
			43·20	189·9	-0·1				62·40*	362·0	0·0

The observations marked with an asterisk are those which were employed in procuring the constants β , E .

The æther which was observed by Dr. Ure below the pressure of 30 inch. appears to have been slightly different in quality from the other; in estimating the comparison this circumstance should be borne in mind.

ON THE CONDITIONS OF THE ATMOSPHERE, AND ON THE CALCULATION OF HEIGHTS BY THE BAROMETER.

The same principles are applicable to the constitution of the atmosphere; but we are far from possessing such extensive and satisfactory data for testing the accuracy of the formulæ. The best observations for this purpose are those of M. Gay Lussac, recorded by M. Biot in the *Connaissance des Temps* for 1841, in the following table.

Table des observations par ordre de hauteurs barométriques.

Numéro des observations par ordre de pression.	Températures en degrés du thermomètre centésimal.	Moyenne des indications des deux hygromètres.	Hauteur moyenne du baromètre dans l'atmosphère, ramenée à celle d'un baromètre à niveau constant.	Hauteurs correspondantes au-dessus de l'Observatoire de Paris, calculées par la formule barométrique de M. Laplace.
			m	m
1	+30.75	57.5	0.76568	0.00
2	12.50	62.0	0.5381	3032.01
3	11.00	50.0	0.5143	3412.11
4	8.50	37.3	0.4968	3691.45
5	10.50	33.0	0.4905	3816.79
6	12.00	30.9	0.4666	4264.65
7	11.00	29.9	0.4626	4327.86
8			0.4528	4511.61
9	8.75	29.4	0.4528	4511.61
10	8.25	27.6	0.4404	4725.90
11	6.50	27.5	0.4353	4808.74
12	5.25	30.1	0.4229	5001.85
13	1.00	33.0	0.4141	5175.06
14	4.25	27.5	0.4114	5267.73
15	2.50	32.7	0.3985	5519.16
16	0.00	35.1	0.3918	5631.65
17	+ 0.50	30.2	0.3901	5674.85
18	- 3.00	32.4	0.3717	6040.70
19	- 1.50	32.1	0.3696	6107.19
20	- 3.25	33.9	0.3670	6143.79
21	- 7.00	34.5	0.3339	6884.14
22	- 9.50		0.3288	6977.47

I shall employ the 1st, 5th, and 21st observations for the determination of the constants, and I propose then to calculate with these constants the temperatures corresponding to the intermediate observations. As the pressures are proportional to the heights of the barometer, if the variation of gravity be neglected we may take the heights of the barometer to represent them, and we have

$$\begin{array}{lll}
 p = \cdot 76568 & \theta = 30^{\circ} \cdot 75 & \frac{1}{\alpha} = 266 \cdot 67 \\
 p' = \cdot 4905 & \theta' = 10^{\circ} \cdot 50 & \frac{1}{\alpha} + \theta' = 277 \cdot 17 \\
 p'' = \cdot 3339 & \theta'' = -7^{\circ} \cdot 00 & \frac{1}{\alpha} + \theta'' = 259 \cdot 67
 \end{array}$$

I find

$$\begin{aligned}
 \frac{\left(\frac{p''}{p}\right)^{\beta} - 1}{\left(\frac{p'}{p}\right)^{\beta} - 1} &= \frac{(\theta'' - \theta) \left(\frac{1}{\alpha} + \theta'\right)}{(\theta' - \theta) \left(\frac{1}{\alpha} + \theta''\right)} \\
 &= [0 \cdot 2988164].
 \end{aligned}$$

The quantity between brackets being the logarithm of the corresponding number

$$\begin{aligned}
 \beta &= -\cdot 32931 & \gamma &= 1 \cdot 4910 \\
 E &= \frac{\left(\frac{p''}{p}\right)^{\beta} \left(\frac{1}{\alpha} + \theta''\right) - \left(\frac{1}{\alpha} + \theta\right)}{\theta'' - \theta} \\
 &= -1 \cdot 1618 & H &= \cdot 53772 \\
 \tau &= \frac{[2 \cdot 8081857]}{p^{-\cdot 32931} + 1 \cdot 1618} - 266^{\circ} \cdot 67
 \end{aligned}$$

in the centigrade scale, the pressure corresponding to $\cdot 76568^m$ of mercury in the barometer being unity. In Fahrenheit's scale,

$$\tau = \frac{[3 \cdot 0634582]}{p^{-\cdot 32931} + 1 \cdot 1618} - 448^{\circ},$$

the pressure corresponding to 30.14 inches of mercury being unity.

If we take $\gamma = 1 \cdot 5$, assuming the 21st observation of M. Gay Lussac, $E = -1 \cdot 1920$.

The difference in the results obtained with these constants from those obtained with the other system of constants $\gamma = 1 \cdot 4910$ and $E = -1 \cdot 1618$, is quite insignificant, only changing the density slightly in the fifth place of decimals. By taking $\gamma = 1 \cdot 5$

$$g' = g(1 - q)^2(1 - Hq),$$

so that the expression for the density becomes more simple, consisting of only three terms, c^{-3u} , c^{-2u} , c^{-u} , (as will be seen hereafter), which is advantageous in the theory of astronomical refractions.

Mr. Russell has calculated for me the following table, by means of my expressions, and with the constants

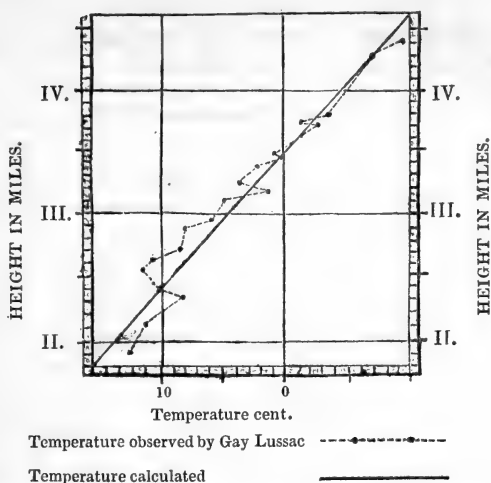
$$\gamma = 1.5, \quad E = -1.192:$$

No.	Observed pressure p .	Calculated height in miles.	Temperature τ .		Density ρ .
			Calculated.	Observed.	Calculated.
1	0.76568	0.000	+30.75	+30.75*	.99999
2	.5381	1.895	14.74	12.50	.74276
3	.5143	2.131	12.68	11.00	.71514
4	.4968	2.310	11.10	8.50	.69473
5	.4905	2.376	10.52	10.50*	.68736
6	.4666	2.632	8.24	12.00	.65928
7	.4626	2.676	7.85	11.00	.65457
8					
9	.4528	2.785	6.87	8.75	.64299
10	.4404	2.926	5.61	8.25	.62828
11	.4353	2.985	4.99	6.50	.62222
12	.4229	3.130	3.67	5.25	.60744
13	.4141	3.236	2.80	1.00	.59692
14	.4114	3.269	2.61	4.25	.59346
15	.3985	3.428	1.05	2.50	.57818
16	.3918	3.512	0.28	0.00	.57010
17	.3901	3.533	+ 0.08	+ 0.50	.56805
18	.3717	3.772	- 2.12	- 3.00	.54576
19	.3696	3.800	- 2.37	- 1.50	.54321
20	.3670	3.835	- 2.70	- 3.25	.54004
21	.3339	4.295	- 7.00	- 7.00*	.49947
22	.3288	4.369	- 7.70	- 9.50	.49317

The observations marked with an asterisk are those which were employed in deducing the constants.

The temperatures calculated by Mr. Russell from my formula may be considered as identical with the "température régularisée par la continuité", given by M. Biot in the *Conn. des Temps*, 1841, p. 13. The observations* of M. Gay Lussac of temperature are represented in the following diagram:

* The irregularities of the observations of temperature in any future ascent might perhaps be diminished if the ballast were suffered to escape gradually in a continued stream.



The abscissa represents the temperature in degrees of the centigrade thermometer, and the ordinate the height in miles.

At first the decrements of temperature are nearly equal for equal increments of altitude. These observations by no means furnish so good a criterion of the accuracy of my formula as the observations which have been made of the temperature of steam and other vapours. The determination of the constants γ and E for the atmosphere must be repeated at some future time; for it is obvious that no great reliance can be placed upon the extreme precision of the values now obtained* until other ascents have been made, and many similar observations have been compared together. We may then hope to obtain constants accurately appertaining to a mean state of the atmosphere, and the variations which take place in their values corresponding to fluctuations of the temperature; the pressure and the humidity of the atmosphere at the earth's surface may then be investigated. M. Biot has suggested that balloons furnished with self-registering instruments should be moored over each of the principal observatories of Europe. This plan appears to me subject to great difficulties. The weight of the line attached will diminish the buoyancy, so that I apprehend it will be found impossible to send up a balloon so fastened to any considerable altitude. The escape of gas will, I imagine, render it very difficult to maintain the balloon at the same height for any length of time. The height of the balloon will also be subject to great variations from the tension of the line changing with the force and direction of the wind. I am disposed to attach much greater

* Dulong found, for atmospheric air, perfectly dry, $\gamma = 1.421$. See Poisson, *Méc.*, tom. ii. p. 646.

value to well-regulated ascents, in which every effort should be made to reach the highest possible altitude; and of course simultaneous observations should be made at the earth's surface at such short intervals of time that every observation of the aéronaut may be comparable with a similar observation at the surface of the earth. As, however, the density and temperature of the atmosphere above the height of five miles from the earth's surface can never be the subject of direct experiment and observation, the observations which can be made upon the conditions of steam and other vapours will always maintain an indirect importance from the light which they throw upon the conditions of the atmosphere. I do not think that an examination of observations made in aërostatic ascents will ever furnish a sure guide to the relation sought between the temperature and the pressure, although if such a relation is furnished by theory and corroborated by observations of other vapours, (which can be carried through a greater extent of the thermometric scale, and, above all, through the low pressures where the variations of temperature become more rapid,) the observations of aéronauts may serve to determine with sufficient accuracy the constants involved in the formula for atmospheric air.

The following table*, calculated by Mr. Russell, shows the density and temperature of the air at different altitudes, calculated by means of my expressions and with the constants

$$\gamma = 1.5, \quad E = -1.192:$$

Height in miles.	Pressure p .		Temperature τ .		Density ϵ .
	Metres.	Inches.	Cent.	Fahr.	
0	0.76568	30.145	+ 30.75	87.35	1.00000
1	.63718	25.087	22.43	72.37	.85614
2	.52741	20.764	13.83	56.89	.73038
3	.43399	17.086	+ 4.94	40.89	.62066
4	.35480	13.968	- 4.23	24.39	.52515
5	.28799	11.336	13.71	+ 7.32	.44223
6	.23189	9.130	23.50	- 10.30	.37042
7	.18506	7.286	33.60	28.48	.30844
8	.14621	5.756	44.05	47.29	.25512
9	.11420	4.496	54.82	66.68	.20942
10	.08806	3.467	65.96	86.73	.17042
15	.01806	0.711	127.32	197.18	.05034
20	.00125	0.049	199.38	326.88	.00720
24	.00000	0.000	-265.95	-446.71	.00000

* In calculating this table, the law of Marriotte and Gay Lussac, expressed by the equation $p = k g (1 + \alpha \theta)$, has been implicitly supposed to hold good throughout: this of course is only conjectural, and it is not intended to attach precision to the temperatures assigned to the great altitudes.

[To be continued.]

XLII. On Magneto-electric Induction; in a Letter to M. Gay-Lussac. By MICHAEL FARADAY, D.C.L., F.R.S.*

MY DEAR SIR,

I BEG to address to you the following pages upon the subject of electro-magnetism, and request the favour of their insertion in the *Annales de Chimie et de Physique*. They may, I fear, provoke a controversy that I would willingly avoid; but under the existing circumstances I feel compelled to adopt the present course of proceeding, for silence, should I maintain it, would be regarded as an admission of error, not only in a philosophical, but also in a moral point of view, from which I believe myself wholly exempt.

You will undoubtedly understand that I allude to the Memoir by Messrs. Nobili and Antinori. I address myself to you, because your judgement was sufficiently favourable to my former memoir for it to obtain a place in your excellent and truly philosophical Journal; and because Messrs. Nobili and Antinori's memoir being also inserted, the *Annales* contain all that has been written upon the subject. I therefore venture to hope that you will not refuse to admit the present article.

On the 24th of November, 1831, my first memoir, which you did me the honour to insert in the *Annales* for the month of May, 1832 (p. 5—69.), was read before the Royal Society; and it was the first announcement that I made of my researches in electricity.

On the 18th of December, 1831, I addressed a letter to my friend M. Hachette, which he was pleased to communicate to the Academy of Sciences on the 26th of the same month †.

[* Translated from the *Annales de Chimie et de Physique*, vol. li. p. 404. Mr. Faraday, in the preface to his collected "Experimental Researches in Electricity," published last year, and reviewed in L. and E. Phil. Mag., vol. xiv. p. 468, refers to several papers of his own, long since published, in the following terms. "Before concluding these lines I would beg leave to make a reference or two; first, to my own papers on Electro-magnetic Rotations in the Quarterly Journal of Science, 1822, xii. 74. 186. 283. 416., and also to my Letter on Magneto-electric Induction in the *Annales de Chimie*, li. p. 404. These might, as to the matter, very properly have appeared in this volume, but they would have interfered with it as a simple reprint of the 'Experimental Researches' of the Philosophical Transactions." As the papers here alluded to are now scarce, and as one of them has appeared in the French language only, we propose to transfer them in succession to our pages, in order that the purchasers of Mr. Faraday's volume may be enabled to possess, in a small compass, the entire series of his researches in electricity and magnetism. We begin with the letter on Magneto-electric induction, addressed to M. Gay-Lussac.—EDIT.] † Vide the *Lycée*, No. 35.

It was also inserted in the *Annales* for December, 1831 (p. 402.). My second series of Researches, bearing date of 21st of December, 1831, was read before the Royal Society on the 12th of January, 1832, and may be found in the *Annales* for June, 1832 (p. 113—162.). These are my only publications upon this subject, up to the present time, excepting a few notes appended to memoirs by other authors; and the whole were written, and publicly read, anterior to any publication on the same subject, by any individual whatever.

During this time, my letter to M. Hachette, which was inserted in the *Annales*, attracted the attention of Messrs. Nobili and Antinori, and those laborious philosophers published a memoir, of the date of 31st of January, 1832, and which was thus posterior to all my publications. This memoir is contained in the *Annales* for December, 1831 (p. 412—430.). A second memoir, entitled “*New Electro-magnetic Experiments*,” by those gentlemen, dated 24th of March, 1832, appeared in the *Annales* for July (p. 280—304.).

My letter to M. Hachette, which in his kindness to me he read before the Academy of Sciences, has, I fear, become a source of error and misunderstanding, and has been productive of injury, rather than benefit, to the cause of philosophic truth. At the same time I know not how to explain my meaning, and place the facts in their proper light, without having the air of complaining in a manner of Messrs. Nobili and Antinori, which to me is particularly disagreeable. I respect those gentlemen, on account of all they have done, not in relation to electricity alone, but for the cause of science in general; and were it not that the contents of their memoirs oblige me to speak, and leave me only the alternative of admitting or denying the exactitude of their assertions, I should have passed unnoticed the scientific errors discoverable in them, leaving to others the task of animadversion. These gentlemen had, unfortunately, no further knowledge of my researches than they gathered from my short letter to M. Hachette, and without taking the trouble to refer to my memoirs, which in these circumstances I cannot but think they ought to have done, they at once misinterpret the sense of an expression relating to M. Arago’s beautiful observations, assume that I had not previously done that for which they take credit to themselves; and finally, they advance what to me appear to be fallacious ideas upon the magneto-electric currents, and present these ideas as *corrections* of mine, though with mine they were as yet unacquainted.

First, allow me to rectify what I regard as the most important mistake of all, the false interpretation given of my

words, for the correction of the errors committed in the experiments might have been left to time.

Messrs. Nobili and Antinori write (*Annales*, vol. xlviii. p. 428.), "Mr. Faraday considers M. Arago's magnetism of rotation to be entirely connected with the phænomenon which he discovered ten years ago. *He ascertained THEN, according to the statement in the notice, that by the rotation of a metallic disc under the influence of a magnet, electric currents may be produced in the direction of the radii of the disc in sufficiently considerable quantities to render the disc a new electrical machine.* We are entirely ignorant how Mr. Faraday discovered this fact, and we know not how a result of this nature *could remain so LONG generally unknown, and, so to speak, FORGOTTEN* in the hands of the discoverer," &c.

Now *I never said* what is here imputed to me. In my letter to M. Hachette, quoted at the head of the notice, I gave a short account of what I had recently discovered, and read before the Royal Society on the 24th of the preceding month. This notice may be found at page 402 of the same number of the *Annales*, and is as follows: "The fourth part of the memoir treats of M. Arago's equally curious and extraordinary experiment, which, as is known, consists in making a metallic disc revolve under the influence of a magnet. Mr. Faraday considers the phænomenon which is manifested in this experiment as *intimately connected* with that of magnetic rotation which he was so fortunate as to observe ten years ago. *He has ascertained that by the rotation of the metallic disc under the influence of a magnet, electric currents may be formed in the direction of the radii of the disc, in sufficient number to render the disc a new electrical machine.*"

I never either said, or intended to say, that I had obtained these electric currents by the rotation of a metallic disc, at an epoch previous to the date of the memoir that I was then engaged in writing; but I said that the extraordinary effect discovered by M. Arago was connected in its nature with the electro-magnetic rotation I had discovered several years before, both being due to a tangential action; and that by the rotation of a disc near a magnet I could (at the time I was writing) cause currents of electricity to escape, or have a tendency to escape in the direction of the radii, thus rendering the disc a new electrical machine; and this I think is fully proved in the part of my memoir of which I gave a sketch: it may be seen in the *Annales*, vol. i. p. 65—118.

I am extremely desirous of explaining this error, because I

have always admired M. Arago's prudence and philosophic reserve, in resisting the temptation to give a theory of the effect which he had discovered until he could offer one perfectly applicable, and in refusing his assent to the imperfect theories of others. Admiring him I adopted his reserve in this respect, and for that reason, perhaps, had my eyes open to recognise the truth as soon as it was presented.

We now arrive at that part of the subject which relates to the philosophy of my memoirs. The fourth part of my memoir of the 24th of November, 1831, contains my opinion on the cause of the phænomenon discovered by M. Arago, an opinion that even now I see no reason to alter. Messrs. Nobili and Antinori, in their papers of the 31st of January and 24th of March, 1832, animadvert upon certain errors which they attribute to me, and enter upon extended developments of magneto-electric phænomena. I cannot, however, discern that they have added a single fact to those contained in my memoirs, unless it be the obtaining of a spark with a common magnet, a result that I had myself previously obtained, but only with the electro-magnet. On the other hand, these gentlemen's memoirs appear to me to contain erroneous ideas upon the nature of magneto-electric currents; they exhibit also mistaken views as to the action and direction of those currents in Arago's revolving disc. They say, "*We have recently verified, extended, and perhaps corrected in some particulars the results of the English philosopher,*" &c. (*Annales*, vol. l. p. 281.) And again at page 298, commenting on what they *suppose* to be my ideas (for though my papers had been read, and were published, they had not thought proper to consult them), they say, "We have already given our opinion upon this idea, but if at the commencement of our researches it were difficult to reconcile it with the nature of the currents discovered by M. Faraday, what can we say after all the new observations that we have made during the progress of our investigations? We say that we have a competent judge in the galvanometer, and that by its means the question must be decided."

With the most sincere desire to be set right when I am in error, I yet find it impossible to discover any corrections in the memoirs of these gentlemen by which I can profit; but I fully admit the competency of the galvanometer, and shall proceed as briefly as possible to submit our different ideas to its decision, in all that relates to Arago's phænomenon: and I am at the present time so satisfied with the facts and results stated in my published memoirs (though were I to rewrite

them I should make alterations in some parts), that it will not be necessary to refer to any experiments that are not therein contained.

It is not my intention further to animadvert upon Messrs. Nobili and Antinori's first memoir. An English translation of it appeared in the *Philosophical Magazine**, to which I added some corrections in the form of notes, copies of which I had the honour of sending to you, and to the authors. My present object is to compare the second part of their publications with the fourth part of my first memoir, and with parts of the other memoirs, as a means of throwing light upon the general principles. The intention of the two articles is to explain Arago's phænomenon, and as fortunately they are both contained in the fiftieth volume of the *Annales*, they may be referred to with facility. The reference to my own papers will be thus, (F. 114.), and to those of Messrs. Nobili and Antinori by a simple indication of the page of the *Annales*.

At page 281, after a few general remarks, we read, "We have recently verified, extended, and perhaps corrected in some particulars the results of the English philosopher; *we then said* that magnetism of rotation found a real support in the new facts developed by Mr. Faraday, and that, consequently, the theory of such magnetism then *appeared* to be so far advanced as fully to merit an effort to develop the physical principles upon which it depends. *It is to this object that the present article is devoted*," &c. Upon this extract I shall only remark, that exactly four months previously I had said the same thing, in the memoir that I read before the Royal Society, and had given, what I hope will prove, a true and exact explanation of the philosophy of the effect under consideration (F. 4—80.).

At page 282 we read, "We have already noticed these currents in our first researches, that is, in the first paper inserted in the Number for December" (p. 412.). But I had "already noticed these currents" four months earlier (F. 90.).

At page 283 are described "galvanometrical explorers or probes," which are nothing more than what I had previously described under the name of *collectors* or *conductors* (F. 86, &c.).

At the commencement of the investigation of the state of Arago's revolving disc adjacent to a magnet, two relative positions of the plate and the magnet are chosen; one called (p. 284.) the "*central arrangement*," in which the magnetic pole is placed vertically to the centre of the disc; and the other (p. 285.) the "*excentric arrangement*," in which the magnet acts beyond that point.

[* *Phil. Mag. and Annals*, N.S. vol. xi, p. 401.]

With regard to the *central arrangement*, we read (p. 284.), "In this case the magnet acting upon the centre of the disc, the probes do not transmit any indication of a current to the galvanometer, *let them be placed where they may*; and if by chance small deviations should be remarked, they arise from imperfect centralization, so that if this *defect be corrected* all indications, &c. of an equivocal source immediately disappear. Indeed what is the result if we employ an electro-dynamic spiral which turns quite round on its own centre, always opposite to the same magnetic pole? *Absolutely nothing*. Its revolution is an *unimportant circumstance*; for the formation of the currents is *wholly due to another condition, they being manifested only at the moment when the spirals are brought near to the magnets, or removed from them*. So long as the spirals are present, whether they move or not, there is no current: so also there is none in the case of central rotation in which the points of the disc remain constantly at the same distance from the magnetic pole, by renewing thus the combination of continued presence, to which Mr. Faraday's *new laws* in relation to currents DO NOT ASSIGN ANY EFFECT."

This assertion is so erroneous in every part, that I have been obliged to quote the passage at full length. In the first place, *there is a tendency* to the formation of currents of electricity in the revolving disc, in the case of "central arrangement," as well as in every other case (F. 149—156.); but their direction is from the centre to the circumference, or *vice versa*, and it is to these parts that the collectors should be applied. It is precisely this which renders the revolving disc a new electrical machine (F. 154.), and it is upon this point that Messrs Nobili and Antinori are so entirely mistaken in their two memoirs. This error is repeated throughout the whole of the memoir that I am now comparing with my first paper, which, if I mistake not, contains the theory of Arago's phenomenon in all its parts.

At page 284 we find, that when a helix turns upon its axis concentrically with a magnetic pole, the result is *absolutely nothing*, and that the condition of rotation is unimportant. Now, though I have not made any experiments on the subject, I venture to assert that there will be an effort in the electric current to pass in a transverse direction to the helix, and that the circumstance of its rotation, instead of being unimportant, is in these cases the only condition essentially requisite for the production of currents. The helix, in fact, may be considered as analogous to a cylinder which might occupy its place, but to which it is very inferior, as it consists of a long coil of wire. It may also be regarded as a simple

wire placed in any situation occupied by a cylinder, and I have shown that they produce currents in their state of rotation, if their opposite extremities are connected with the galvanometer.

It is said at page 284, that the formation of currents is "*wholly due to another condition*, they being manifested only at the moment when the spirals are brought near to the magnets, or removed from them. So long as the spirals are present, whether they move or not, *there is no current*." So also there is none in the case of *central rotation*," &c. Now in my first paper I showed that the essential condition was not the approximation or removal of the metal in movement, but simply that it should intersect the magnetic curves (F. 101. 116. 118. &c.); and that consequently, *cæteris paribus*, the movement without change of distance is the most effective and powerful means of obtaining the current, instead of being the condition in which the result is absolutely nothing. In my second paper I *proved* that a movement through the magnetic curves was the only condition necessary (F. 217.); and that so far from the approximation or removal of the metal being necessary, currents may be produced in the magnet itself, merely by moving it in the proper direction (F. 220.).

Lastly, when treating of this "central arrangement," and the supposed absence of effect when "the points of the disc remain constantly at the same distance from the magnetic pole," Messrs. Nobili and Antinori say (p. 285.), "by thus renewing the combination of continued presence to which Mr. Faraday's *new laws* in relation to currents *do not assign any effect*;" and in a note we read, "These laws are reduced to three," which are specified, at first fully, and then in a more condensed form, as follows: "FIRST LAW. During gradual approximation: the current produced contrary to the current producing; repulsion between the two systems. SECOND LAW. The distance unvarying. No effect. THIRD LAW. During recession. The current produced in the same direction as the current producing. Attraction between the two systems." I have never myself given these as the simple laws which govern the production of the currents that I was so fortunate as to discover; nor do I understand how Messrs. Nobili and Antinori can say that they are *my* laws, though at page 282 one of them is so called. But I described these three cases together in my first memoir (F. 26. 39. 53.), as well as in the notice, that is, in my letter to M. Hachette, as effects that I had observed. It has been established, by what I have already said, that they are not the laws of the action of magnetic electricity, for the simple fact that cur-

rents of electricity may be obtained by means of the revolution of a cylinder (F. 219.), or of a disc in connection with a magnet (F. 218.), or of the magnet itself (F. 220.), disproves each of them. ONE LAW, which includes all the effects, is given in my memoir (F. 114. 116. &c.), and it simply expresses the direction in which the moving conductor intersects the magnetic curves. This law of direction being given, I endeavoured to recapitulate the whole in the terms that I shall here repeat (F. 118.).

“All these results show that the power of inducing electric currents is circumferentially excited by a *magnetic resultant* or *axis of power*, just as circumferential magnetism is dependent upon and is exhibited by an electric current.”

I have quoted this passage of the Italian physicists at full length, because it contains nearly all our points of difference, both as to fact and opinion, concerning this part of the subject. Having thus shown all the errors included in it, I shall endeavour to be more concise, while I exhibit, *with the assistance of the galvanometer*, such others, derived from them, as are dispersed over the remainder of the memoir. It is indeed curious to remark, how, with galvanometrical indications generally correct, these gentlemen have suffered themselves to be led astray under the influence of preconceived opinions. For example, at page 287—288, and in fig. 2. plate iii. is shown the result of an examination by the galvanometer of the currents in a revolving disc. These currents are indicated nearly correctly by means of arrows; but the *two consequences* deduced from them agree with the theory enunciated, and are diametrically opposed to the facts.

“The immediate inspection of the arrows which mark the currents in the two regions of the disc (fig. 2.) leads to one of these consequences (p. 287.), and it is *that a system of currents is developed upon the parts that enter contrary to those produced on the other side*. The other consequence arises from comparing the currents produced upon the disc with the currents of the producing cause, and it is *that the direction of the currents upon the parts that enter is contrary to that of the producing currents, while on the other side the direction in the two systems is identical*.”

But I showed in my first memoir (F. 119.), that “when a piece of metal is passed either before a single pole, or between the two opposite poles of a magnet, or near electromagnetic poles, whether ferruginous or not, electric currents are produced across the metal transverse to the direction of motion. This fact is proved by means of wires (F. 109.), plates (F. 101.) and discs (F. 92. &c.); and in all these cases

the electric current was in the *same direction*, whether the metal were brought near to, or caused to recede from the magnet, provided that the direction of its movements were unaltered. In Arago's revolving disc the electricity that I was able to obtain from one of these parts in a multitude of experiments always agreed with these results (F. 92. 95. 96.), and consequently (F. 119. &c.) I recapitulated them in a short description, as presented in Arago's disc, establishing more particularly (F. 123.), that the currents produced near or under the poles are discharged or return into the parts of the metal situated on each side of and more distant from the place of the pole, where the magnetic induction is necessarily "weaker."

[To be continued.]

XLIII. *On the Detection and Estimation of Colophony (common Rosin) when dissolved in the Fixed Oils.* By J. DENHAM SMITH, Esq.

To Richard Phillips, Esq., F.R.S., L. & E., &c.

MY DEAR SIR,

SOME samples of linseed oil were sent to me for analysis in July last, the bulks of which had been exported, but were found on their arrival to be unsaleable and perfectly useless, for when mixed with white-lead in the usual manner for making paint, the mixture became quite hard at the expiration of a few hours, it having set as plaster of Paris does when moistened with water.

At first sight it was obvious that all these oils had been considerably adulterated; for not only was their colour much deeper than that of ordinary linseed oil, but they were all extremely viscid, resembling castor oil, in this respect, much more than the comparatively thin and fluid commercial linseed oil. Suspecting both from smell and taste, particularly the latter, that the adulterant was common colophony (black rosin), I endeavoured to ascertain whether my conjecture was well-founded, and if so, to determine the proportion of colophony contained in the several samples; especially as the adulterated oils were likely to become the subject of legal proceedings.

I am not aware that there is any mode on record for separating, or even detecting common rosin when dissolved in the fixed oils, so that I was obliged to make numerous experiments, before a method was discovered which appeared

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to offer satisfactory results. It would be useless to describe these unsuccessful trials; I therefore at once proceed to explain the mode I ultimately adopted for detecting and estimating the proportion of colophony contained in the various specimens of oil.

I found that when colophony was dissolved in pyroxylic spirit, the solution gave a bulky white precipitate, on the addition of acetate of lead also dissolved in pyroxylic spirit; but that when unadulterated commercial linseed oil was digested with this solvent, the clear solution when cold was merely rendered turbid on the addition of the spirituous solution of acetate of lead. Discovering that pyroxylic spirit exerted a very marked solvent action on the precipitate above-mentioned, and that the supernatant, or filtered solution, gradually deposited more of this precipitate, after standing for some time or when gently evaporated, I had recourse to the common rectified spirit of wine, about sp. gr. $\cdot 832$, which although it is capable of dissolving this compound of resin and oxide of lead, yet does so in almost an inappreciable quantity, especially if the spirit be cold.

Having dissolved 30 grs. of common rosin in a small portion of linseed oil, by the assistance of heat, about 3 fluid ounces of rectified spirit of wine were poured upon the oil, and thoroughly mixed with it by agitation; this was boiled for two or three minutes, and then allowed to cool and the oil to subside; the next day the clear spirituous solution was poured off, about the same quantity of fresh spirit added to the residual oil, and the mixture agitated and boiled as before; when bright this solution was decanted and mixed with the former, the remaining oil again treated with about an ounce of spirit, and this solution added to the others; a fourth time spirit was added, but this solution gave no precipitate with acetate of lead; this agent merely rendering it turbid, as in the case of pure linseed oil when dissolved in spirit; from this I concluded that the whole of the resin, which the rectified spirit was capable of separating, was extracted from the oil. The three solutions when mixed were of a light yellow colour, and perfectly clear, when on the addition of a freshly prepared solution of acetate of lead in rectified spirit the characteristic bulky white precipitate fell. At the expiration of four-and-twenty hours this was collected on a filter weighing 11.7 grs., and washed with cold rectified spirit until the washing left the merest perceptible stain when evaporated in a porcelain dish; I then dried the precipitate on folds of bibulous paper, and finally with a gentle heat, until it suffered no decrease of weight, when the weight of the pre-

precipitate and filter was found to be 29·4 grs. — 11·7 grs., weight of filter, = 17·7 grs. of the compound of resin and oxide of lead obtained from 30 grs. of common colophony, which would indicate that 59 grs. of this compound are equal to 100 grs. of rosin.

This experiment was repeated with 40 grs. of rosin dissolved in a little oil, and this mixture treated in the manner above described. In this instance 26·7 grs. of the precipitate were obtained, which is equal to 66·7 per cent. of the resin employed. It appears, from these experiments, that although this method is incapable of affording strictly accurate results, I presume from the slight solvent action of the spirit upon the precipitate, yet that an approximation to the quantity of colophony contained in oils adulterated with this substance, may be obtained, which perhaps by some subsequent modifications of and precautions in the process, may lead to a mode capable of yielding not merely a tolerable approximation, but rigidly accurate results.

When unadulterated commercial linseed oil is treated with rectified spirit in the manner described, the alcoholic solution affords no precipitate on the addition of a spirituous solution of acetate of lead, but it merely becomes turbid, exhibiting appearances very different from the bulky precipitate which is produced if colophony be present. The samples of adulterated oil when submitted to the process I have described, respectively afforded 27·7, 21 and 26·3 per cent. of this compound of resin and oxide of lead, which would indicate, according to the average of the two experiments with resin and oil already mentioned, 44·1, 33·4 and 41·7 per cent. of common rosin respectively contained in these specimens of oil.

When this white precipitate is suspended in rectified spirit, and a current of hydrosulphuric acid gas passed through it, decomposition takes place, sulphuret of lead is formed, and a light yellow-coloured solution, which reddens litmus, is obtained; this, on evaporation, leaves a brown and brittle residuum exactly resembling common rosin. For this mode of separating the acid resin from the oxide of lead, I am indebted to my friend Dr. Brett. From the circumstance of this alcoholic solution affording no precipitate with nitrate of silver until the addition of a little ammonia, I conclude the resin acid which is combined with the oxide of lead in the precipitate obtained in the foregoing experiments, to be the sylvic acid. When ignited in a covered crucible, so as to avoid the access of atmospheric air, and consequent oxidation of the lead, I find that the precipitate, whether obtained from

the two first experiments, or whether reduced from the precipitates afforded by the adulterated oils submitted to examination, produces precisely the same quantity of metallic lead, viz. 27 per cent. Both these precipitates also, when exposed to a moderate heat, fuse, affording a brown transparent substance very much resembling common colophony, but which seems to be harder and more brittle than rosin is. Pure linseed oil, when mixed with the same quantity of rosin as analysis indicated in one of the samples of adulterated oil marked "Raw oil," that containing 44.1 per cent., was exactly of the same density as the sample in question, both being .982, whilst pure linseed oil was considerably lighter, its density being .9518; thus corroborating not merely the fact of adulteration, but also the close approximation to correct results afforded by the mode of analysis adopted.

I remain, my dear Sir, yours very truly,

Duke-Street, Liverpool,
Sept. 3, 1840.

J. DENHAM SMITH.

XLIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 149.]

April 30, 1840.— **T**HE following communications were read:—

1. A Letter from Sir John Barrow, Bart., V.P., addressed to the President, accompanying a series of Magnetic Observations made on shore, and on board Her Majesty's ships 'Erebus' and 'Terror,' under the direction of Captain James Clark Ross, R.N., together with a Series of Observations made on the temperature and specific gravity of the ocean at various depths, and at the surface, namely,

"Observations of the magnetic intensity on shore, and on board H.M.S. Erebus, with needle F. 1.

"Magnetic dip observations on shore, and on board H.M.S. Erebus, with needle F. 1.

"Observations for the magnetic dip on shore, and on board H.M.S. Terror.

"Observations of the magnetic dip by needle F. C. 5. on shore, and on board H.M.S. Terror.

"Observations in magnetic intensity by needle F. C. 5. on shore, and on board H.M.S. Terror."

The whole of these observations are up to the 31st December, 1839. They are transmitted to the Royal Society from the Lords Commissioners of the Admiralty.

2. Postscript to Major Sabine's paper, entitled "Contributions to Terrestrial Magnetism," which was read at the meeting of March 19th; (see p. 144), containing an extract from a letter from Capt. James Clark Ross, commanding the Antarctic expedition, dated from St. Helena, February 9th, 1840; noticing the success which had

attended the employment of Mr. Fox's instrument, in observations of the magnetic dip and intensity on shipboard.

3. "A few remarks on a Rain Table and Map," drawn up by Joseph Atkinson, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

The table and map which accompany this paper exhibit the average annual depth of rain falling in different places in Great Britain.

4. "Extracts from a Meteorological Journal kept at Allenheads, in the county of Northumberland," by the Rev. W. Walton, F.R.S.

The general result of these observations, which were recorded twice each day, namely, at 9 A.M., and at 3 P.M., during the whole of the year 1839, is, that the mean temperature taken at those times was $44^{\circ} 8'$; the mean height of the barometer, corrected and reduced to the temperature of 32° , was 28.401 inches, and the quantity of rain in the year was 55.71 inches. The author subjoins several remarks on the conclusions deducible from an examination of the tables.

5. "Description of an Astronomical Clock invented by the late Captain Henry Kater, F.R.S.," drawn up from his own memorandums by his son Edward Kater, Esq. Communicated by Sir John F. W. Herschel, Bart., V.P.R.S.

The great object aimed at by Captain Kater in the construction of the escapement of a chronometer, is to communicate equal impulses to the pendulum through some principle perfect in itself, and not dependent for its success on superior execution. In the escapement invented by him, the pendulum merely raises a weight, and is impelled by that weight through an increased space in its descent. It neither unlocks a detent, nor has anything to do with the train; and as the weight raised, and the spaces described, are constant quantities, this escapement is, in the strict meaning of the term, one of equal impulse.

May 7.—A paper was read, entitled "Researches in Embryology, Third Series: a Contribution to the Physiology of Cells." By Martin Barry, M.D., F.R.S., F.R.S.E., Fellow of the Royal College of Physicians in Edinburgh. Of this paper an abstract was given in our number for June, vol. xvi., p. 526.

A paper was also read, entitled "On the Odour accompanying Electricity, and on the probability of its dependence on the presence of a new substance;" by C. F. Schönbein, Professor of Chemistry, Bâle, communicated in a letter to Michael Faraday, Esq., D.C.L., F.R.S., &c.

The author's attention having been long directed to the remarkable fact, that odour, resembling that of phosphorus, is given off during the escape of positive electricity from the point of a conductor into air; and is likewise perceptible when lightning has struck any object, and also when water is electrolyzed, he has investigated the circumstances attending these phenomena; and the results he has obtained will, he expects, afford a clue to the discovery of their cause.

The odour which accompanies the electrolyzation of water, he observes, is only disengaged at the positive electrode. He also finds that the odoriferous principle can be preserved in well-closed glass bottles for any length of time. The only metals which yield this odour are gold and platina; but dilute sulphuric, phosphoric, and nitric acids, and from aqueous solutions of several of the salts, also disengage it. Raising the temperature of the fluid to the boiling point prevents the odour from arising; and the addition of comparatively small quantities of powdered charcoal, iron, zinc, tin, lead, antimony, bismuth or arsenic, or of a few drops of mercury, to the odorous principle contained in a bottle, immediately destroys the smell; and the same happens when platina or gold, heated red-hot, is introduced into the vessel containing that volatile substance.

May 14.—A paper was read, entitled, “Tables of the Variation, through a cycle of nine years, of the mean height of the Barometer, mean Temperature, and depth of Rain, as connected with the prevailing Winds, influenced in their direction by the occurrence of the Lunar Apsides, with some concluding observations on the result.” By Luke Howard, Esq., F.R.S., &c.

From the Tables here given, the author draws the following conclusions:—

1. The barometer is higher under the lunar apogee, than under the perigee; the mean height in the former case being 29·84517, and in the latter, 29·75542.

2. The mean temperature is lower under the apogee than under the perigee; that of the former being 48°·7126, and of the latter, 49°·0356. The mean of the whole year was 48°·7126.

3. The rain of the weeks following the apsis exceeds that under the perigee; but with two striking exceptions in the annual result of nine years, the one in the wettest, and the other in the driest year of the cycle.

With regard to the winds, the author remarks that those from the north, north-east, and east, prevailed under the apogee on 38 days, under the perigee on 21 days; and those from the south, south-west, and west, prevailed under the apogee on 20 days, under the perigee on 38 days.

It appears, therefore, that in the climate of London, the moon in her perigee brings over us the southern atmosphere, which tends to lower the density and raise the temperature of the air, occasioning also a larger precipitation of rain. In the apogee, on the contrary, there is a freer influx of air from the northward, a higher barometer, a lower temperature, and less rain; subject, however, to a large addition of rain under this apsis twice in a cycle of nine years, at the times when also the extremes of wet and dry take place on the whole amount of the year.

A paper was also read entitled, “Experimental Researches into the strength of Pillars of Cast Iron, and other materials.” By Eaton Hodgkinson, Esq. Communicated by Peter Barlow, Esq., F.R.S., &c.

The author finds that in all long pillars of the same dimensions,

the resistance to crushing by flexure is about three times greater when the ends of the pillars are flat, than when they are rounded. A long uniform cast-iron pillar, with its ends firmly fixed, whether by means of disks or otherwise, has the same power to resist breaking as a pillar of the same diameter, and half the length, with the ends rounded, or turned so that the force would pass through the axis. The strength of a pillar with one end round and the other flat, is the arithmetical mean between that of a pillar of the same dimensions with both ends round, and one with both ends flat. Some additional strength is given to a pillar by enlarging its diameter in the middle part.

The author next investigated the strength of long cast-iron pillars with relation to their diameter and length. He concludes that the index of the power of the diameter, to which the strength is proportional, is 3.736. He then proceeds to determine, by a comparison of experimental results, the inverse power of the length to which the strength of the pillar is proportional. The highest value of this power is 1.914, the lowest, 1.537, the mean of all the comparisons, 1.7117. He thus deduces, first, approximate empirical formulæ for the breaking weight of solid pillars, and then proceeds to deduce more correct methods of determining their strength.

Experiments on hollow pillars of cast iron are then described, and formulæ representing the strength of such pillars are deduced from these experiments.

After giving some results of experiments still in progress for determining the power of cast-iron pillars to resist long-continued pressure, the author proceeds to determine from his experiments the strength of pillars of wrought iron and timber, as dependent on their dimensions. The conclusion for wrought iron is, that the strength varies inversely as the square of the pillar's length, and directly as the power 3.75 of its diameter, the latter being nearly identical with the result obtained for cast iron; for timber, the strength varies nearly as the 4th power of the side of the square forming the section of the pillar. Experiments for determining the relation of the strength to the length in pillars of timber, were not instituted, as, from the great flexure of the material, it was considered that no very satisfactory conclusions on this point could be derived experimentally.

In conclusion, the author gives the relative strengths of long pillars of cast iron, wrought iron, steel, and timber.

May 21.—The following papers were read, viz.:

1. "Remarks on the Meteorological Observations made at Alten, Finmarken, by Mr. S. H. Thomas in the years 1837, 1838, and 1839." By Major Sabine, R.A., V.P.R.S., and Lieut. Col. Sykes, F.R.S.; being a Report from the Committee of Physics, including Meteorology, to the Council, and communicated by the Council to the Royal Society.

These observations, made at Alten in lat. $69^{\circ} 58' 3''$ N., and $23^{\circ} 43' 10''$ east of Paris, would seem to have a claim to the attention of the Royal Society, as they offer the *experimentum crucis* of

Professor Forbes's empirical formula respecting the gradual diminution of the daily oscillations of the barometer, within certain limit hours, from the equator to the poles. Professor Forbes has laid down an assumed curve, in which the diurnal oscillation amounts to $\cdot 1190$ at the equator and 0 in lat. $64^{\circ} 8' N.$, and *beyond that latitude* the tide should occur *with a contrary sign*, plus becoming minus. Now Alten being nearly in lat. 70° , if Professor Forbes's law hold good, the maxima of the diurnal oscillations should occur at the hour for the minima at the equator, and a similar inversion should take place with respect to the minima. Mr. Thomas has himself however modified the value his observations would otherwise have had, by adopting 2 P.M., instead of 3 P.M., for the hour of his observations for the fall; and he has adapted his barometrical observations to a mean temperature of 50° Fahr., instead of 32° . The first year's observations commence on the 1st October, 1837, and terminate on the 30th September, 1838. The barometer stood 66 feet 5 inches above low-water mark, and the thermometer hung at 6 feet above the ground; but care was not always taken to prevent the sun shining on it. The mean height of the barometer for the year was $29^{\circ} 771$, and the mean of the thermometer almost coincident with the freezing point, viz., $32^{\circ} 017$. The maximum height of the barometer was $30^{\circ} 89$ in January, and the minimum $28^{\circ} 71$ in October. The mean of the barometer at 9 A.M. was $29^{\circ} 764$, therm. $33^{\circ} 455$; at 2 P.M. $29^{\circ} 765$, therm. $33^{\circ} 327$; and at 9 P.M. $29^{\circ} 784$, therm. $29^{\circ} 270$. The diurnal observations would seem to support Professor Forbes's theory; but the 9 P.M. observations are entirely opposed to it, as they appear with the same maximum sign as at the equator, whereas the sign ought to have been the reverse; indeed, with respect to the diurnal observations, the mean of five months of the year at 9 A.M. gives a plus sign, although the mean of the year at 2 P.M. only gives the trifling quantity of $\cdot 001$ plus. There is one remarkable feature in these observations that cannot fail to strike the meteorologist. M. Arago, from nine years' observations at Paris, reduced to the level of the sea, makes the annual mean height $29^{\circ} 9546$; twenty-one years' observations at Madras make it $29^{\circ} 958$; and three years' observations at Calcutta, by Mr. James Prinsep, make it $29^{\circ} 764$; and Mr. Thomas brings out $29^{\circ} 771$. That there should be this coincidence between the observations at Calcutta and Alten is curious. Neither Mr. Thomas nor Mr. Prinsep state whether or not their means are reduced to the level of the sea. It is to be suspected they are not.

For the next year, that is to say, from Oct. 1838 to Sept. 1839, both inclusive, Mr. Thomas uses a French barometer and French measurements, with centigrade thermometer attached to the barometer, and Fahrenheit's for the detached thermometer. He changes his time of observation from 9 A.M. to 8 A.M., 2 P.M., and 8 P.M., and he reduces his barometrical observations to 0 centigrade. The results of the year are as follow:—mean annual pressure $29^{\circ} 627$ English; thermometer Fahr. $33^{\circ} 36$; greatest pressure

in April, least in January!! The mean of 8 A.M. is $29^{\circ}620$; therm. $33^{\circ}75$. The mean of 2 P.M. is $29^{\circ}631$; therm. $34^{\circ}73$; and at 8 P.M. $29^{\circ}631$; therm. $30^{\circ}57$. The diurnal observations assist to support Professor Forbes's theory; but as in the preceding year, the P.M. observation is at fault; and if the hour had been 9 o'clock instead of 8 o'clock, it would probably have been more so than it appears. The low annual mean state of the barometer for the year 1837-38 is even increased in the last year's observations; and as fresh instruments* appear to have been used, there is ground to believe that the fact is associated with the locality, and it may be desirable not only to record in the Proceedings of the Royal Society the data already supplied, but to recommend to Mr. Thomas more particular inquiry on the subject.

The phenomena of the Aurora Borealis appear to have been observed by Mr. Thomas with great assiduity, and recorded with great care. On examining the register, with reference to M. Erman's important remark, that "in Siberia two kinds of aurora are distinguished, one having its centre in the west, and the other in the east, the latter being the more brilliant," it is found that twenty-two nights occur in the course of the two winters in which the formation of arches of the aurora is noticed and their direction recorded; of these, *ten* are to the *west*, having their centres rather to the southward of west, the arches extending from N.W. to S.S.E. and S.E.; *seven* are to the *east*, or more precisely to the southward of east, the arches extending from N.E. to S.E. and S.W. Of the five others, *four* are said to be from east to west across the zenith, and cannot therefore be classed with either of the preceding, and *one* is noticed generally as being to the north. The facts here recorded appear to afford an evidence of the same nature as those mentioned by M. Erman, as far as regards there being two centres of the phenomena. In respect to the relative brilliancy of the eastern and western aurora, nothing very decided can be inferred from the register. If, as M. Erman supposes, they may be referred respectively to "les deux foyers magnétiques de l'hémisphère boréal," it is proper to notice that the position of Alten is nearly midway between those localities.

There can be no doubt that the frequent appearance of the aurora, and the peculiarities of the phenomena observed there, render it a most desirable quarter for a magnetical and meteorological observatory.

EDWARD SABINE.

W. H. SYKES.

2. "Second Letter on the Electrolysis of Secondary Compounds, addressed to Michael Faraday, Esq., D.C.L., F.R.S., &c." By J. Frederic Daniell, Esq., For. Sec. R.S., Professor of Chemistry in King's College, London.

The author, in this letter, prosecutes the inquiry he had commenced in the former one, [of which an abstract appeared in the

* It appears that the barometer was compared before leaving France, and subsequently to its being taken back to that country.

L. & E. Phil. Mag., vol. xv., p. 317.—EDIT.] into the mode in which the chemical elements group themselves together to constitute *radicles*, or proximate principles. He considers his experiments as establishing the principle that, considered as electrolytes, the inorganic oxy-acid salts must be regarded as compounds of metals, or of that extraordinary compound of nitrogen and four equivalents of hydrogen to which Berzelius has given the name of *ammonium*, and compound anions, chlorine, iodine, &c., of the Haloid salts; and as showing that this evidence goes far to establish experimentally the hypothesis originally brought forward by Davy, of the general analogy in the constitution of all salts, whether derived from oxy-acids or hydro-acids. Some remarks are made on the subject of nomenclature, and the rest of the paper is occupied with the details of the experiments, all bearing on the important subject which he has undertaken to investigate.

May 28.—The following papers were read, viz.:

1. "Meteorological Register kept at Port Arthur, Van Diemen's Land, during the year 1838, and Register of Tides at Port Arthur, from August 1838 to July 1839, both inclusive." By Deputy-Assistant-Commissary-General Lempriere. Communicated by Sir John Franklin, R.N., F.R.S., &c.

2. "Notice relative to the form of the Blood-particles of the *Ornithorhynchus hystrix*." By John Davy, M.D., F.R.S.

A portion of the blood of the *Ornithorhynchus hystrix*, mixed when fresh with a strong solution of common salt, being examined by the author, exhibited a few globules of irregular shape. Another portion, preserved in syrup, contained numerous globules, most of which had an irregular form, but many were circular; none, however, were elliptical, like those of birds. Hence the author concludes, that in form they accord more with those of Mammalia.

3. "Researches on Electro-chemical equivalents, and on a supposed discrepancy between some of them and the atomic weight of the same bodies, as deduced from the theory of isomorphism." By Lieut.-Colonel P. Yorke. Communicated by Michael Faraday, Esq., D.C.L., F.R.S., &c.

The author describes various experiments made with a view to determine the electro-chemical equivalents of sodium and potassium. Three experiments gave, respectively, 22.3, 22.9, and 25, as the equivalent of the former; and two other experiments gave, respectively, 45 and 41.7, as the equivalent of the latter of these substances. He then inquires what would be the result of the electrolyzation of the aqueous solutions of soda and potash, on the hypothesis of these bodies being composed of two equivalents, or atoms, of metal, and one of oxygen. To determine this question he employs a solution of dichloride of copper in muriatic acid, as being a substance composed of two atoms of metal and one of an electro-negative element. Its electrolysis gave as the equivalent of copper, 52.8, 59.4, and 61.6, numbers approximating closely to 63.2, or double the atomic weight of copper. After a long train of investigation, he concludes that there is no reason deducible from the

theory of isomorphism for doubting the correctness of the received atomic weights of silver, sodium, &c., but that the difficulty, or anomaly, if it may be so called, should be considered as attaching itself to the di-compounds of copper; and that Faraday's propositions on this subject remain unimpeached.

4. "Second series of Approximate Deductions made from about 50,000 observations taken during the years 1836, 1837, and 1838, at the Port Louis Observatory, Mauritius, four times each day; namely, at 8 A.M., at noon, and at 4 and 8 P.M." By J. A. Lloyd, Esq., F.R.S.

5. "On the Solubility of Silica by Steam; with an account of an experiment on the subject, conducted in the East Indies by Julius Jeffreys, late of the Hon. East India Company's Medical Establishment."

The inner surfaces of a flue built of siliceous bricks appeared to be deeply eroded by the passage over it of steam at a very high temperature, and fragments of siliceous materials laid in the course of the current were partially consumed. A siliceous crust was deposited on several vessels of stone ware, coated with a micaceous glaze, placed in the upper part of the furnace, and this crust was re-dissolved when the vessels were removed to a hotter situation in the same furnace. The author notices the experiments of Dr. Turner* and others, which failed in showing the solubility of silica by steam, in consequence, as he conceives, of the heat having not been sufficiently great to effect the solution.

June 4.—A paper was read, entitled, "Contributions to the Chemical History of Archil and of Litmus." By Robert Kane, M.D., M.R.I.A. Communicated by Francis Baily, Esq., V.P.R.S.

After a preliminary sketch of the labours of Heeren and of Robiquet in investigating the origin of the beautiful colouring materials termed *Archil* and *Litmus*, obtained from different kinds of colourless lichens, and their detection of the two proximate principles termed *erythrine* and *orceine*, the author states the object of the inquiries detailed in the present paper to be threefold; viz. first, to ascertain the primitive form of the colour-making substance in a given species of lichen, and trace the stages through which it passes before the coloured substance is developed; secondly, to determine the nature of the various colouring substances which exist in the archil of commerce; and thirdly, to examine the colouring materials of ordinary litmus. He finds in the lichen *Roccella tinctoria* the following bodies, either pre-existing in the plant, or formed during the processes employed for its analysis: 1. Erythryline; 2. Erythrine (the Pseudo-erythrine of Heeren); 3. Erythrine bitter; 4. Telerythrine; and 5. Roccelline (the Roccellic acid of Heeren). The properties and constitution of these substances are then described, and the chemical formulæ given, which are deducible from their respective analyses. The author finds the archil of commerce to consist essentially of three ingredients, namely, orceine, erythrolic acid, and

[* See L. & E. Phil. Mag. vol. v. p. 297.—EDIT.]

azoerythrine; of each of the two former there exist two modifications, and there is, in addition, a yellow matter. After comparing his results with those obtained by Heeren, by an examination of the products evolved by his erythrine in contact with air and with ammonia, and stating reasons for some changes in nomenclature, the author gives the chemical formulæ resulting from his own analysis of these different substances.

His inquiries into the constitution of ordinary litmus, which form the last division of his subject, lead him to the conclusion that that substance contains the principles designated by him as Erythrolein, Erythrolitmine, Azolitmine, and Spaniolitmine; and that the colouring constituents of litmus are, in their natural condition, red; the blue substances being produced by combination with a base, which bases in that of commerce are lime, potass, and ammonia; and there is mixed up in the mass a considerable quantity of chalk and sand. The details of the analyses of these several substances, and the resulting chemical formulæ representing their constitution, are then given.

The concluding section of the paper is occupied by an inquiry into the decoloration of the bodies which exist in archil and in litmus. The latter of these, the author concludes, is reddened by acids, in consequence of their removing the loosely combined ammonia by which the blue colour is produced; and the so-called hydrogen acids liberate the colouring matter by their combining with the alkali to form bodies (either chlorides or iodides), with which the colouring matter has no tendency to unite. Hence it appears that the reddening of litmus is no proof that chloride of hydrogen is an acid, and that the double decomposition which occurs is the same in principle, whether hydrogen or a fixed metal come into play. After detailing the blanching effects of other deoxydizing agents on the colouring matter of litmus, and the action of chlorine on orceine and azolitmine, the author remarks, that in these actions chlorine is subjected to conditions different from those which determine the nature of the results with the generality of organic bodies, and that the displacement of hydrogen, so marked in other cases, does not exist in the class of substances under consideration; but that, in reality, the products of the bleaching energy of chlorine resemble in constitution the compounds of chlorine which possess bleaching powers.

A paper was also read, entitled, "On the Corpuscles of the Blood." By Martin Barry, M.D., F.R.S.

The author in the course of his researches in Embryology, detailed in his "third series," observed that some of the corpuscles of the blood undergo progressive alterations in their structure. The corpuscles so altered he believes to be of the same kind as those described by Professor Owen; and having found that the alterations in question terminate in a separation of the corpuscles into globules, he thinks this fact confirms the idea of Professor Owen—that the blood-disc undergoes spontaneous subdivision. The author farther observed, that the corpuscles of the blood, in certain altered states, undergo rapid and incessant changes of form, which cannot be traced

to the action of neighbouring cilia. A corpuscle will sometimes assume the figure of an hour-glass, as if it were preparing to divide itself into two parts, but it instantaneously either regains its previous form, or assumes a new one. These motions are incessant, and so rapid, that it is not easy to catch and delineate any of the resulting forms; they are compared to the writhings of an animal in pain. The author has seen them in a rabbit, as late as two hours and a half after death, and thinks it probable that they may continue for a longer time, although, when under the microscope, they gradually and in a short time cease; the rapid changes of form, which are at first apparent, passing into gentle undulations, and being succeeded by an alternation of rest and motion*.

Should these facts be thought to confirm the opinion of John Hunter, that the blood "has life within itself," or "acquires it in the act of forming organic bodies," because its corpuscles in certain states exhibit "vital actions," still his assertion that "the red globules" are the least important part of the blood, will appear to have no just foundation.

The author finds that the phenomena attending what is called "vital turgescence" of the blood-vessels, depend not merely on an accumulation and stagnation of blood, but on changes in the condition of its corpuscles, which assume a more or less globular, or elliptical appearance resembling cells. Their interior is dark, from a great increase of red colouring matter which accumulates around a pellucid and colourless point, corresponding in situation to that of the central part of nuclei in other cases; and so completely do the corpuscles fill their vessels, that the fluid portion of the blood is excluded, and the corpuscles are compressed into polyhedral forms. This condition of the blood-corpuscles during vital turgescence of the vessels, the author thinks deserving of consideration, in connexion with many of the phenomena attending local accumulations of blood, both in health and in disease; and more especially with reference to increased pulsation, the exudation of colourless fluid, and the heat and redness of inflamed parts.

According to the views of the author, the formation and nourishment of organs is not effected merely by the fluid portion of the blood, for he has discovered that the cells which he showed in his "Third Series of Researches in Embryology†" form the chorion, are altered blood-corpuscles; and he has farther found that muscular fibre (that is, the future muscle-cylinder, not the fibril) is formed by the coalescence of cells, which also are derived from corpuscles of the blood. He has seen and figured every stage of transition, from the unaltered blood-corpuscle to the branched cells forming the chorion, on the one hand, and to the elliptical or oblong muscle-cells, on the other. The colour is not changed, except that the blood-corpuscles, when passing into cells for the formation of mus-

* [See a note on this subject by Dr. Barry, p. 157 of the present volume.—EDIT.]

† [See Lond. and Ed. Phil. Mag. vol. xvi. p. 526.—EDIT.]

cle, become of a much deeper red. There seems to occur in these an increase of red colouring matter.

Valentin, in describing the mode of the formation of muscle, had stated that globules approach one another and coalesce to form threads, which in many places have the appearance of a necklace, but subsequently lose the traces of division, and become cylinders. Schwann had conjectured that the globules just referred to—as having been observed by Valentin—are cells, and that these cells coalesce to form a secondary cell, that is, the muscle-cylinder. The author confirms the observations of Valentin and the conjectures of Schwann, with the addition, that the globules coalescing to form the muscle-cylinder are blood-corpuscles which have become cells. The fibrils appear to be subsequently formed within the cylinder, which thus becomes the muscular fasciculus. The medullary portion of the cylinder appears to be composed of the pellucid objects, one of which is contained within each altered blood-corpuscle. Some of these pellucid objects, however, continue to occupy a peripheral situation.

The author thinks it is not probable that muscular fibre and the chorion are the only tissues formed by the corpuscles of the blood; he is disposed rather to inquire, how many are the tissues which they do not form? Nerves, for instance, are known to arise very much in the same manner as muscle-cylinders; and epithelium-cells sometimes present appearances which have almost suggested to the author the idea that they were altered corpuscles of the blood.

Schwann had previously shown that “for all the elementary parts of organisms there is a common principle of development,”—the elementary parts of tissues having a like origin in cells, however different the functions of those tissues. The facts made known in the present memoir not only afford evidence of the justness of the views of Schwann, but they farther show that objects, such as the corpuscles of the blood, having all the same appearance, enter immediately into the formation of tissues which physiologically are extremely different. Some of these corpuscles arrange themselves into muscle, and others become metamorphosed into constituent parts of the chorion. But the author thinks it is not more difficult to conceive corpuscles having the same colour, form, and general appearance, undergoing transformations for very different purposes, than to admit the fact made known by two of his preceding memoirs,—namely, that the nucleus of a cell, having a central situation in the group which constitutes the germ, is developed into the whole embryo, while the nuclei of cells occupying less central situations in the group, form no more than a minute portion of the amnion. It is known that in the bee-hive a grub is taken—for a special purpose—from among those born as workers, which it perfectly resembles until nourished with peculiar food, when its development takes a different course from that of every other individual in the hive.

The Society then adjourned over the Whitsun Recess, to meet again on the 18th of June.

GEOLOGICAL SOCIETY.

Feb. 1, 1840.—Annual General Meeting.

The President announced that the Wollaston Medal had been awarded to Prof. Dumont, of Liége, for his Memoir, Map, and Sections on the Geological Constitution of the Province of Liége, published in 1832; and one year's interest of the Wollaston Fund to Mr. James De Carle Sowerby, in order to facilitate the continuation of his researches in Mineral Conchology; Dr. Buckland, on presenting the Medal to Dr. Fitton, who had been requested by M. Dumont to receive it on his behalf, said:—

DR. FITTON,

I am highly gratified that it has become my duty on the present occasion, to commit to your care as the Representative of our common friend, Professor Dumont, the Wollaston Gold Medal, which has been awarded to him by the Council of this Society for his Memoir on the Geological Constitution of the province of Liége published at Brussels in 1832.

The grounds of our tardy recognition in 1840, of the merits of a work published so long as eight years ago, are the same, that in 1830, prompted the Judges appointed by the Academy of Brussels, to select this Memoir as most worthy of the Prize then proposed by that Academy, for the best Geological description of the province which has formed the subject of M. Dumont's successful labours.

In the work thus doubly crowned, the Author has described the mineralogical and zoological characters of the rocks which occupy this district, and determined in minute detail, the relative places in order of succession, and the superficial extent of each subordinate division of the several formations. He has also illustrated the same by an accurately coloured Geological Map, and by coloured Sections, showing the general disposal of the strata in their original order of deposition, and the extraordinary derangements and disturbances that have subsequently thrown them into a state of almost inextricable confusion. In the execution of this work, M. Dumont has evidenced unusual powers of discriminating and accurate observation, combined with a high capacity of reducing the minutiae of local details under the dominion of enlarged and masterly theoretical generalizations. Advancing at the early age of twenty one, to a task of gigantic labour, in a region where the unexampled disturbances, and almost incredible complexity of its component strata had baffled the sagacity of the most experienced geologists, this extraordinary youth at once withdraws the veil of confusion which had hitherto disguised the stratigraphical arrangements of his native province, and as it were, by an intuitive touch, reduces to order the entangled and almost incredible phænomena of dislocation, contortion, and inversion which had perplexed his predecessors in the same field of observation.

In addition to the scientific value of M. Dumont's exact and laborious researches, in illustrating a high and difficult problem in positive geology, his work assumes a place of great statistical and

commercial importance, as describing the structure and contents of a rich and productive carboniferous district containing eighty-three beds of valuable coal; and its practical utility has been fully shown, by the fact of a second edition having been required to supply the demands of the landed proprietors, and persons practically interested in the operations and products of the coal mines.

The geological tribunal of Brussels, including the highly distinguished geologist Omalius d'Halloy, at once appreciated duly, and rewarded as they deserved, these brilliant discoveries; but the phenomena represented on M. Dumont's map and sections were so unusually complex and improbable, that the geologists of England could not but forbear to admit their reality, until it was fully confirmed by our personal examination, with the aid of that new light which M. Dumont's discoveries had thrown upon them. The result of such inquiry has been a full corroboration of M. Dumont's representations, and at this late hour we at length come forward with the homage of our tardy but sincere acknowledgements; a duty too long delayed, from the exercise of precaution in its administration, but for this very reason now become more urgent, when the grounds for conscientiously discharging it have passed the ordeal of severe and critical investigation. It is for this great work then on the geological constitution of the Province of Liège, such as in 1832 it issued from the hands of a young, and then unknown individual, and apart from any more recent attempts to identify the Belgian formations with those of England, that our Society has awarded to M. André Hubert Dumont their Gold Wollaston Medal for the present year; in testimony of their admiration of the almost precocious talents then displayed by him, and of their sense of his worthiness to fill the distinguished scientific position to which he is now advanced, as Professor of Mineralogy and Geology in the College of Liège*.

Dr. Fitton, on receiving the Medal from the hands of the President, said, that he had been requested by M. Dumont to express his great regret that unavoidable duties prevented his appearing in person on this occasion. M. Dumont's letter states with deep feeling his sense of the honour which the Geological Society of London has thus conferred upon him, and his hope that he may soon be enabled to come into England, for the purpose of extending his personal acquaintance with the members of this Society, and of being enabled, with the aid of their knowledge, to perfect the comparison of the ancient strata of Belgium with those of this country. The Society could not but anticipate great advantage to Geology from the application of M. Dumont's talents to the comparative inquiries to which his letter alludes.

On presenting the prize awarded to Mr. James De Carle Sowerby, Dr. Buckland said:—

It is with no small pleasure that I rise to perform the duty of

* [A paper by M. Dumont "On the Equivalents of the Cambrian and Silurian Systems in Belgium," will be found in *Lond. and Edinb. Phil. Mag.*, vol. xv. p. 146.—*EDIT.*]

placing into your hands the award that has been made to you by the Council of the Geological Society, of one year's interest of the Wollaston Fund, in order to facilitate the continuation of your researches in Mineral Conchology.—The services are great which have been rendered to Geology by the extremely useful and well-timed work on fossil shells, which was many years ago begun by your excellent father, and continued by him to the end of his life, and has been since conducted by yourself; and the association of his name with that of Dr. Wollaston, recalls to my mind, as it must to the minds of most of my hearers, pleasing and grateful recollections of the benefits which during their lives they both conferred on this Society, and which their works will have extended to all our contemporaries and successors in this department of scientific inquiry. It was your father's peculiar merit to be one of those accurate and enthusiastic observers of nature, who have in modern times contributed so much to remove from science the rugged and austere aspect under which it used to be presented; and who by facilitating to every one the means of advancing pleasantly in its pursuit, have, in an essential manner, promoted, and given popularity to the study of Botany and Conchology.

It is to Mineral Conchology, which he so especially promoted, that we who are occupied with the investigation of the structure of the earth, have in modern times been mainly indebted for evidences which have led to the establishment of many of the most important stratigraphical distributions, that have been founded on the successive changes in animated nature which are made known to us by the study of fossil shells. It was on this foundation that Cuvier and Brongniart established their important divisions of the marine and freshwater strata of the Tertiary formations, which have since been more minutely distributed by Mr. Lyell into the eocene, pliocene, and miocene series, according to their relative numbers of extinct and recent species of fossil shells. It was on a similar foundation that Mr. William Smith rested his identification of the Secondary strata of England. It is on the same basis of conchological evidence that Mr. Murchison has founded his fourfold subdivisions of the Silurian portion of the Transition rocks; and it is chiefly to the illumination which this branch of Palæontology has shed upon the changes that took place on the surface of the earth, whilst its strata were in process of formation, that we owe the rapid advances in geological knowledge which have been made since the commencement of the present century. To this rapid progress, arising from the introduction of the evidences of mineral Conchology, your own publications and those of your family have largely contributed; you have further co-operated materially in advancing our inquiries by your personal assistance, at all times cheerfully and liberally rendered, to all your fellow labourers in the same fields of scientific research, who stood in need of your aid, for the elucidation of minute distinctions in the characters of fossil organic remains, which have at this time become so important an element in geology. The volumes of the Transactions of this Society, and other publi-

cations by many of its Members, including myself, bear further testimony to the importance of your labours, in illustrating our works with drawings and engravings of fossil shells and plants, expressing their characters with a degree of accuracy and truth, which no pencil or burine but those of a scientific artist could possibly accomplish; and I am sure I give utterance to the feelings of all our fellows now around me, when I thus publicly acknowledge the services you have rendered both to ourselves, and to the science we cultivate; and express the satisfaction with which we thus publicly recognise the value of your exertions.

Mr. Sowerby then expressed himself in the following terms:—

SIR,

I hardly know what to say, so deeply do I feel the unexpected and kind award bestowed upon me by this Society, but I must assure you, that I am extremely grateful for the honour done me. When, Sir, you spoke of my father, you excited feelings most dear to me, and I have long felt that I have experienced more consideration than I have deserved, in consequence of the esteem that has ever been attached to his memory. But I must have been a most ungrateful son had I not, after his persevering and kind instructions, done something for the advancement of Natural History. What little I have performed, especially for Members of this Society, has been for the love of Science; and I feel far more than amply rewarded by the honourable present I have just received at your hands. You have stated, Sir, that you take a pleasure in associating the name of Wollaston with that of Sowerby; I shall never forget the kindness and patience with which Dr. Wollaston communicated information. When the reflective goniometer was first completed by him, he spent several hours one morning with me in his study measuring the cleavages of various minerals related to hornblende and augite which I took to him for his opinion; and at another time he indulged me with an equally long lesson on the chemical examination of minute portions of minerals. Little did I think at that time that I should ever share encouragement continued by his bounty, after his departure from this world; but I have lived to feel that his benevolence lives beyond the grave.

Sir, I receive this award as a trust reposed in me, and hope that I shall not be found wanting in carrying out the object the Council has in view.

I beg sincerely to thank the Society for the confidence placed in me.

The following Fellows were declared to have been elected the Officers and Council for the ensuing year.

President.—Rev. W. Buckland, D.D. Professor of Geology and Mineralogy in the University of Oxford.

Vice-Presidents.—G. B. Greenough, Esq. F.R.S. & L.S.; Leonard Horner, Esq. F.R.S. L. & E.; Sir Woodbine Parish, K.C.H. F.R.S.; Rev. William Whewell, B.D. F.R.S. Professor of Casuistry in the University of Cambridge.

Secretaries.—Charles Darwin, Esq. F.R.S.; William John Hamilton, Esq.

Foreign Secretary.—H. T. De la Beche, Esq. F.R.S. & L.S.

Treasurer.—John Taylor, Esq. F.R.S. & L.S.

Council.—Arthur Aikin, Esq. F.L.S.; Francis Baily, Esq. F.R.S. L.S.; Viscount Cole, M.P. F.R.S.; W. H. Fitton, M.D. F.R.S. L.S.; W. Hopkins, Esq. M.A. F.R.S.; R. Hutton, Esq. M.P. M.R.I.A.; Charles Lyell, Esq. F.R.S. L.S.; William H. Miller, Esq. M.A. Professor of Mineralogy in the University of Cambridge; R. I. Murchison, Esq. F.R.S. L.S.; E. W. W. Pendarves, Esq. M.P. F.R.S.; Philip Pusey, Esq. M.P. F.R.S.; George Rennie, Esq. F.R.S.; Daniel Sharpe, Esq. F.L.S.; Rev. Adam Sedgwick, F.R.S. L.S. Woodwardian Professor in the University of Cambridge.

Address to the Geological Society, delivered at the Anniversary, on the 21st of February, 1840, by the REV. PROFESSOR BUCKLAND, D.D., F.R.S., Corresponding Member of the Institute of France, President of the Society.

GENTLEMEN,

By the Report just read, you have seen that the state of our Society is one of steady and salutary progression; forty-three new Members have been added to the List of our Fellows, from which seventeen have been removed by death or resignation, leaving our actual number 768, with an increase of twenty-six during the last year. The vacancies that have occurred upon our foreign list have been supplied by three highly distinguished cultivators of science on the Continent, each pre-eminent for his successful labours in high departments of our subject, namely:

Major Puillon de Boblaye, in Positive Geology,

Professor Adolphe Brongniart, in Vegetable Palæontology,

Professor Gustave Rose, in Crystallography and Mineral Analysis.

We are rich in property, though our funds are, at this moment, low, but they will speedily be repaired by the sale of two large and costly parts which have been added to our Transactions.

The Reports of the Library and Collections in our Museum are satisfactory. The chief additions to the former consist of presents from Authors and Members of the Society. Our principal benefactor has been Mr. Greenough, who has given us a Collection of the older Authors,—supplying many of our deficiencies in the Literature of Geology and Mineralogy. Considerable progress has been made in the arrangement of the Cabinets by our Sub-Curator, Mr. Woodward, under the superintendence and directions of Mr. Lonsdale; one hundred and sixty drawers of rock specimens and fossil remains having been labelled, and in part catalogued, since the meeting of last year. It is satisfactory to find that the number of persons who come to study our Collections has been much increased.

Our entire establishment continues to receive the inestimable advantages it has long enjoyed, from the zealous superintendence, and scientific acquirements of our Curator, Mr. Lonsdale.

Our Wollaston Medal has been awarded to Professor Dumont, for his Map, Sections and Memoir on the Geological Constitution

of the Province of Liége, published in 1832; and one year's interest of the Wollaston Fund has been presented to Mr. James De Carle Sowerby, to facilitate the continuation of his researches in Mineral Conchology.

More than a quarter of a century has now elapsed since I became a Member of this Society; and fifteen years have passed since I was first placed, by your kindness, in the honourable position of filling this Chair, at that important period of our history when we received the national recognition of a Royal Charter. I shall never cease to consider it one of the brightest rewards of my labours in geology, that my name is enrolled in that charter, as the first President of the Society in its corporate capacity.

Since that important epoch, our chartered body has received from the Government of the country the valuable sanction and advantage of an establishment in the very convenient apartments of Somerset House, which we now occupy. The number and character of the scientific labourers who have joined our ranks, and the volumes added to our Transactions, since these events, show that such encouragements have not been conferred on a society disposed to slumber under the sunshine of prosperity; but that, aided by these advantages, we have endeavoured to maintain a steadily progressive course, in the great work of illustrating the physical structure of the earth.

It is not my duty, on the present occasion, to notice geological memoirs or subjects which belong to years preceding that wherein I entered upon my present office. The usual practice rather confines me to the most remarkable events of the last twelve months, during which I have had the honour to fill this chair.

MUSEUM OF ŒCONOMIC GEOLOGY.

Among the most important of these events, we recognise with gratitude, and confident anticipation of great advantage, both to science and the arts, the establishment, by Her Majesty's Government, of an institution hitherto unknown in England, namely, a Museum of ŒCONOMIC GEOLOGY. This is to be freely accessible to the public at stated periods, in the Department of Her Majesty's Woods and Forests, and Public Works, for the express object of exhibiting the practical application of geology to the useful purposes of life. In this Museum a large store of valuable materials has already been collected and arranged, chiefly by the exertions, and under the direction of Mr. De la Beche. In it will be exhibited examples of Metallic Ores, Ornamental Marbles, Building-stones and Limestones, Granites, Porphyries, Slates, Clays, Marls, Brickearths, and Minerals of every kind produced in this country, that are of pecuniary value, and applicable to the arts of life. Information upon such subjects, thus readily and gratuitously accessible, will be of the utmost practical importance to the miner and the mechanic, the builder and the architect, the engineer, the whole mining interest, and the landed proprietors. The establishment will contain also examples of the results of Metallur-

gic processes obtained from the furnace and the laboratory, with a collection of Models of the most improved machinery, chiefly employed in mining. A well-stored Laboratory is attached to this department, conducted by the distinguished analytical chemist, Mr. Richard Phillips, whose duty it already is, at a fixed and moderate charge, to conduct the analysis of metallic ores, and other minerals and soils submitted to him by the owners of mines or proprietors of land, who may wish for authentic information upon such matters.

The pupils in this laboratory are already actively employed in learning the arts of mineral analysis, and the various metallurgic processes.

A second department in the Economic Museum will be assigned to the promotion of improvements in Agriculture, and will contain sections of strata, with specimens of soils, sub-soils, and of the rocks from the decomposition of which they have been produced.

To this last-mentioned collection proprietors of land are solicited to contribute from their estates labelled examples of soils, with their respective sub-soils; and all persons who wish for an analysis of any sterile soil, for the purpose of giving it fertility, by the artificial addition of ingredients with which nature had not supplied it, may here obtain, at a moderate cost, an exact knowledge of its composition, which may point out the corrective additions which it requires. This portion of the Museum will more especially exhibit the relations of geology to agriculture, in so far as a knowledge of the materials composing the sub-strata may afford extensive means of permanent improvement to the surface.

[To be continued.]

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xvi. p. 148.]

Jan. 10, 1840.—The following communications were read:—

Ephemeris of the Comet now visible. By Mr. C. Rumker, of Hamburg. Communicated by Dr. Lee.

A Letter from Mr. Henry Lawson to the Secretary describing the appearance of the Comet, as seen at Hereford.

The comet was observed by Mr. Lawson on the mornings of the 23d and 29th of December, and of the 8th January instant. It had a tolerably well-defined nucleus, with a brushy tail on the side opposite to the sun. The nucleus subtended an angle nearly equal to half the visual angle subtended by *Jupiter*; and the tail filled the whole field of view, the diameter of which was three minutes of time.

Apparent Positions of the Comet observed at Edinburgh. By Professor Henderson.

Observations of the Comet made at Ashurst and Dulwich. By Robert Snow, Esq.

Mr. Snow found the comet on the 28th of December. The observed diameter of the head was then 58'', and the tail extended beyond the field of view. It was again observed on the 29th, and

also on the 5th and 6th of the present month, when it was very bright and easily found. The nucleus was large, but not stellar.

Occultations of Stars by the Moon to the end of 1839. By Robert Snow, Esq.

Catalogue of the *Pleiades*. By Robert Snow, Esq.

The author states that this catalogue does not lay claim to strict accuracy, but was constructed in order to form a chart, which might be consulted with advantage when occultations of stars in the *Pleiades* by the moon take place. Piazzì's stars falling within the limits of the chart were taken as standards, and the differences between them and the other stars determined by a wire micrometer. For some stars, too faint to allow of illumination, the ring micrometer was used; but they were more usually put down by estimation, which may be done with nicety when many are in the field together. This communication was accompanied by a chart.

On the Variability of a *Cassiopeia*. By Robert Snow, Esq.

In the Monthly Notice for May last (vol. iv. p. 195.), the attention of the Society was directed to the supposed variability of this star; and it has, accordingly, been watched, with the naked eye, since June 9, 1839, up to the present time, January 8, 1840. The relative brightnesses of α , β , γ *Cassiopeia* have been registered on sixty-eight evenings. The result at present is that γ has been generally put down as brightest, and never faintest; β generally as faintest; α faintest twelve times out of sixty-eight, and generally so about the 5th day of the month. In the Society's Catalogue, the order of magnitude is β , $\alpha = \gamma$. Mr. Snow remarks that the star α appears to his eye at all times sharper and better defined than γ or β ; and it is also more readily obscured by fog or haze, although it is a reddish star.

Observations of a *Cassiopeia* in 1831 and 1832. By Mr. W. R. Birt, Librarian and Assistant Secretary to the Metropolitan Institution. Communicated by the President.

These observations were commenced in April 1831, and extend to November 1832; and the earlier part of them, from April to December 1831, have already, with some others, been communicated to the Society. See Monthly Notices, vol. ii. No. 11. In a letter to Sir John Herschel, Mr. Birt states, that since 1832 his attention had not been directed to this star until he read the Monthly Notice for May last, when it immediately occurred to him that his observations might probably assist in determining the period in which the brightness of the star completes the circle of its gradations. When Mr. Birt commenced observing the star in April 1831, the lustre appeared to be at its minimum. In December of the same year, he again observed it to be less than β . His observations were then discontinued until June 1832, when it again appeared less than β . Taking the extreme observations, we have thus two periods completed in about fifteen months, or one period in about 225 days. Assuming this as the period of the variation, and computing from April 26, 1831, the number of days elapsed until April 28, 1839, is 2924, which gives thirteen periods of about 225 days each. Sir John Herschel's

observations give the maximum from November 12, 1838, to January 22, 1839; and calculating from July 7, 1831, twelve periods, the maximum would be obtained on December 4, 1838. On the whole, Mr. Birt concludes that the period of 225 days may be regarded as a first approximation, which may receive correction from a comparison with the earlier observations of Sir William Herschel.

On the Variability and Periodic Nature of the Star α *Orionis*. By Sir John F. W. Herschel, Bart. President.

"In a communication which was read to this Society on the 10th of May last, I pointed out the star α *Cassiopeiæ* as variable and periodical. That the fluctuations in splendour of this star should have escaped general notice is not extraordinary, since the difference between its greatest and least brightness can hardly be estimated at more than half a magnitude. But that a periodical variation to a very much greater extent, in so important and remarkable a star as α *Orionis*, should, up to this time, have been completely unnoticed by astronomers, does appear to me, I confess, not a little extraordinary, and might be taken as an argument to show, more than any thing, the comparatively neglected state of this highly interesting branch of Physical Astronomy. Perhaps, however, in this, as in many other cases, the very prominence of the object has been the cause of its being neglected; as it might easily be supposed by any one entering on this research, that had a star so familiar to every practical astronomer presented any striking peculiarity of this kind, it could not *but* have been observed. Hence, while the attention of observers has been directed, and with success, to much inferior stars, it seems to have been taken for granted, that among stars of the first magnitude nothing, in fact, remained to be discovered.

"Having bestowed much attention, during my residence at the Cape, on the estimation of the magnitudes of the southern stars, both by direct photometrical measurements, assigning numerical values to about sixty or seventy of them, selected as offering convenient gradations of brightness, and also by very assiduous and often-repeated comparisons by the naked eye, with the view to completing a graduated scale down to the fifth magnitude, at least, it became important to connect these magnitudes by similar comparisons with those of the northern hemisphere, by means of stars in the vicinity of the equator admitting of observation at both stations. My method in these observations has been invariably on each night to establish, in the first instance, a sort of skeleton-scale, beginning with the stars of the first magnitude actually visible, and extending as far as was judged convenient for the occasion, then filling in this scale by the insertion of fresh stars between the members. The stars of the first magnitude actually above the horizon at the time of commencing observation were first arranged, and others of that magnitude inserted among them as they rose and gained altitude.

"On the very clear and brilliant night of the 26th November last, being engaged in a process of this kind, I was surprised, and I may almost say startled, by the extraordinary splendour of α *Orionis*,

which far exceeded my idea at the moment of what was its natural state. Proceeding to compare it with other stars of the first magnitude (*Sirius* being, of course, out of the question), their arrangement for the night was found to be as follows :

Capella || α Orionis | Rigel || Procyon ||| Aldebaran | Pollux.

“ In this and subsequent arrangements of the same sort, the number of vertical strokes between the names indicates the estimated amount of interval, or the *grades* or steps of magnitude by which the stars differ. Thus, the step from *Capella* to α *Orionis* is a great one ; that from α *Orionis* to *Rigel*, a distinct but moderate one ; from *Rigel* to *Procyon*, a great one, admitting of the easy insertion of a star decidedly inferior to one, and superior to the other, between them ; from *Procyon* to *Aldebaran*, a very great step, admitting, at least, two such insertions of imaginary stars decidedly diverse in lustre between them ; and so on. Now as I distinctly recollected having, on a great many occasions, placed α *Orionis* nearly on a par with *Aldebaran*, there could be no doubt of a change. Referring next morning to my father’s Catalogues of Comparative Brightness, I find that he makes the star in question slightly inferior, or at most equal, to *Procyon*, and much greater than *Aldebaran*.

“ In consequence of this observation, I proceeded forthwith to draw out in order all the comparisons of α *Orionis* with other stars made at the Cape, on the voyage homewards, and since my return. In so doing, I must confess I was hardly less surprised than at the sight of the star itself to find in my star-lists, containing the results of a partial reduction and arrangement of my Cape observations, α *Orionis* not merely marked as *variable*, but distinct entries made of it in that list at its maximum and minimum,—the maximum being stated as above *Rigel*, the minimum below *Aldebaran*. This, however, had entirely escaped my memory, but being thus recalled, and so forcibly corroborated, I resolved to watch the star more narrowly in future ; the more especially as it seemed to follow, from the tenour of the observations, that its diminution of brightness was likely to be rapid : and so, in fact, it has proved to be.”

The author then proceeds to give the observations on which the evidence of the former changes of the star is grounded. They extend over the years 1836, 1837, 1838, and 1839, and are as follows (denoting, for brevity, α *Orionis* by the word *Orion*) :—

1836.

March 22. Rigel, Procyon, α Crucis, Orion, Regulus, Pollux.

Nov. 12. { Orion, } Procyon, Achernar, α Crucis, Aldebaran, Pollux.
 { Rigel, }

13. Orion = Rigel.

26. Rigel, Orion, Achernar.

1837.

Oct. 24. Orion (high), Achernar, Orion (low), Rigel, Aldebaran.

Dec. 16. Rigel, Achernar, Orion.

29. Rigel, Achernar, Procyon, Orion, Aldebaran.

1838.

- Jan. 2. Rigel, Procyon, Achernar, Orion, Pollux, α Crucis.
 6. Rigel, Procyon, Achernar, Orion, Aldebaran, α Crucis, Pollux.
 13. Rigel, Procyon, Achernar, Orion, Aldebaran, α Crucis.
 Feb. 25. Rigel, Procyon, Orion, α Crucis, Pollux, Regulus.
 April 14. Procyon, Rigel, Orion, Aldebaran, Pollux, Regulus.

1839.

- Jan. 17. Procyon, Aldebaran, Orion, Pollux, Regulus.
 22. Rigel, Procyon, Aldebaran, Orion, Pollux, Regulus.
 Nov. 26. Orion, Rigel, Procyon, Aldebaran, Pollux.

On examining the above series, the general order of arrangement (leaving out Orion) is found to be *Rigel, Procyon, Achernar, Aldebaran, α Crucis, Pollux, Regulus*; and the instances in which the arrangement is different are accounted for by some peculiar circumstances connected with the observations. Thus, with respect to the observation of October 24, 1837, the author states that the misplacement of *Achernar* is accountable for by the circumstance of the two comparisons of *Orion* having been made (as appears by the notices *high* and *low*) first when rising with *Achernar* then high, and *Rigel* low; and at a later period of the night with *Rigel* then high, and, consequently, *Achernar* low. On January 2, 1838, α *Crucis* is set down as inferior to *Pollux*; but these two stars are difficult of comparison, both from situation and difference in colour, and from being, in fact, not very different in lustre. The transposition of *Procyon* and *Rigel* in the observation of the 14th of April, 1838, is unaccountable, except from some unsuspected partial haziness in that part of the sky. This observation was made at sea.

With regard to *Orion*, the observations evidently show three maxima, viz. in Nov. 1836, Oct. 1837, and Nov. 1839; and also three minima, viz. those of March 1836, Jan. 1838, and Jan. 1839. "Reasoning from this, the most obvious conclusion is that of an annual, or nearly annual period. But in that case, we must admit the decrease to be comparatively sudden, and the increase slow; whereas, if we admit of a period of about six months, this supposition will not be necessary, and as the star cannot be observed (for this purpose) in the summer months, there is no *primâ facie* reason against adopting the latter period; respecting which, however, further observation will soon enlighten us."

The observations subsequent to Nov. 26, 1839, confirmed the expected decrease of the star in a very decided manner:—

1839. Nov. 30. Rigel | Orion, Procyon || Aldebaran.
 Dec. 11. Rigel | Orion | Procyon || Aldebaran.
 29. Rigel, Procyon, Orion, Aldebaran.
 1840. Jan. 2. Rigel || Procyon | Orion || Aldebaran.
 5. Rigel || Procyon | Orion || Aldebaran.
 6. Rigel || Procyon || Orion || Aldebaran.

In a note to this last observation, it is stated that "the difference between *Orion* and *Aldebaran* is evidently and rapidly on the decrease. The stars are all high, at nearly equal altitudes, and admirably arranged for comparison."

Jan. 7, 1840. "*Procyon, Orion, Aldebaran*, form a succession by nearly equal steps."—"Upon the whole, I think it may be stated, that in the interval from November 26 to the present date (January 8), *Orion* has sustained a loss of nearly half its light. It may easily be supposed that a diminution, thus evidently still in rapid progress, will, in no long time, carry down the rank of this star below that of *Aldebaran*, and that the confirmation or disappointment of this expectation is awaited with no small interest."

The author concludes with the following remarks:—

"The subject of variable and periodical stars has been of late rather unaccountably suffered to lie dormant; a state of neglect in which, as I have already observed, it ought not to be suffered to remain, and from which I have endeavoured to rescue it on two former recent occasions, by pointing out the stars *α Hydrae* and *α Cassiopeiae*, both large and conspicuous stars, as belonging to the latter class. A periodical change, however, existing to so great an extent in so large and brilliant a star as *α Orionis*, cannot fail to awaken attention to the subject, and to revive the consideration of those speculations respecting the possibility of a change in the lustre of our sun itself which were put forth by my father. If there really be a community of nature between the sun and fixed stars, every proof that we obtain of the extensive prevalence of such periodical changes in those remote bodies, adds to the probability of finding something of the kind nearer home. It is only in comparatively very recent meteorological observations that we can expect to find that precision in the determination of temperatures which is necessary to establish the absence or presence of periodical change in the intensity of solar radiation; and if the period be not annual (as there is no reason why it should be), the usual mode of combining observations of temperature followed by meteorologists is altogether inappropriate to the research, which can only be carried on either analytically, by the introduction of a periodical term with unknown coefficient, epoch, and period, or graphically, by projecting in a continuous curve the *mean daily* temperatures during a long series of years. For the detection of a period of great length, extending over more than a year, the *continued* observation of the temperature of the water a few feet below the surface in open sea, under the equator, on the principles pointed out by M. Arago in his instructions for the voyage of the *Bonite*, would suffice. But we are far from possessing as yet sufficient records of such observations to be worth discussing in this point of view. Such observations must of their nature be casual. Even granting that in every ship which traversed the equator the requisite observations were made, the identity of their thermometric standards would be still open to question.

"The assiduous observation in fixed physical observatories of the temperature of the earth, at several depths below the surface, extending from three to thirty feet—an element which we know to be (in its mean amount) *solely* dependent on solar radiation—would be in every respect more immediately and practically applicable to the inquiry, and we may expect to see it carried out into

effect. The direct measure of the solar radiation, too, by the actinometer*, ought by no means to be neglected in this inquiry.

"M. Poisson, in a late memoir, has considered the possible consequences, in a geological point of view, of the sun and solar system having, in long by-gone ages, passed through a region in which the actual *temperature of space* should be much greater than in its present locality†. The great authority justly attributed to every idea thrown out by this philosopher, must render it a matter of diffidence and difficulty to maintain a contrary view. Without, however, as a matter of abstract speculation, denying this possibility, I would observe that the temperature at any given point of space can arise only from two sources: 1st, That of the æther, as a fluid susceptible of increase and diminution of temperature; and, 2ndly, The radiation of the stars. Of the temperature of the æther as a fluid, I confess I have no conception. Of the existence of such a fluid as the efficient cause of *light*, we have demonstrable evidence. But the properties of *heat* are so linked and interwoven with those of *light*, that it is asking more than can be granted to demand our admission that the æther is a fluid *capable of being heated and cooled*, while it is yet undecided (with a leaning to the affirmative side) whether it be not the *efficient cause* of heat itself.

"As regards the radiation of the stars.—There is a region in the heavens where starlight is decidedly more dense than elsewhere—the milky way. And we have, I may almost say, ocular evidence that our system is excentrically situated within that zone, and nearer to its southern than to its northern portion. Granting a perfect transparency of the celestial spaces, the brightness of any given region of the sky must be alike at all distances, whether we conceive that brightness to be uniformly diffused over its surface, or to emanate from a finite number of undistinguishably small points. Now, although the brightness of the southern regions of the milky way may, for argument's sake, be admitted to be three or four times that of the northern, yet, as that light is *almost* completely obliterated by the presence of a full moon in *any* part of the sky above the horizon, it follows that the brightness of the *general firmament* to a spectator placed within the brightest part of the milky way (supposing him not within the range of an individual *sun*), must be less than that of (*not the full moon itself, but*) that general illumination which the moon communicates to the whole sky by atmospheric reflexion; i. e. an almost infinitesimal quantity compared to the direct light of the lunar disc, the intensity of which can hardly be to that of the sun in a higher ratio than one to half a million.

"The brightest regions in the sky—i. e. the brightest spaces

* "This instrument was devised by me for the dynamical measure of the solar radiation in the spring of 1824; and I have had it in use ever since, with continually increasing confidence in its indications." [See p. 78 of the present volume.—EDIT.]

[† A Translation of M. Poisson's memoir will be found in the SCIENTIFIC MEMOIRS, vol. i. p. 122.—EDIT.]

having a visible area—are those occupied by the planetary nebulae. Of these, there is none which can be compared to *Uranus* in intrinsic brightness, to say nothing of the moon. Supposing, then, our system to be suddenly plunged into the bosom of one of these nebulae, an increase of temperature would take place less than that which would arise from superadding to our own that which the surface of *Uranus* receives from the sun, or less than the 400th part of that which we actually receive from it; and this supposes *Uranus* to reflect *all* the light incident on it.

“Leaving to others to judge, however, how far these arguments are to be considered as militating against the view of climatological changes in remote antiquity above alluded to, I may remark that it is a matter of observed fact, that many stars *have* undergone in past ages, within the records of astronomical history, very extensive changes in apparent lustre, without a change of distance adequate to producing such an effect. If our sun were ever *intrinsically* much brighter than at present, the mean temperature of the surface of our globe would, of course, be proportionally greater. I speak now not of periodical, but of secular changes. But the argument is complicated with the consideration of the possibly imperfect transparency of the celestial spaces, and with the cause of that imperfect transparency, which may be due to material non-luminous particles diffused irregularly in patches analogous to nebulae, but of greater extent—to *cosmical clouds*, in short—of whose existence we have, I think, some indication in the singular and apparently capricious phenomena of temporary stars, and perhaps in the recent extraordinary sudden increase and hardly less sudden diminution of *η Argus*.”

Elements of the comet visible at this period, computed by Dr. Petersen, and received from Prof. Schumacher; and parabolic elements of the same computed by Prof. Henderson, are given in the Monthly Notice for January.

XLV. Intelligence and Miscellaneous Articles.

DETECTION OF IODATE OF POTASH IN IODIDE OF POTASSIUM.

BY MAURICE SCANLAN, ESQ.

IT appears that hydriodic acid is sometimes exhibited as a therapeutic agent, and the method resorted to for its extemporaneous preparation is that recommended by Dr. Andrew Buchanan, of Glasgow. It consists in mixing together, in proper proportion, iodide of potassium and tartaric acid, both in solution.

Now, the quantity of free iodine liberated from this salt, which I have under examination, when treated with tartaric acid, in the way just mentioned, has led some dispensing chemists to suppose that it contains more iodine than other specimens of iodide of potassium, which, when treated in a similar way, afford a solution that is colourless, or, at most, of a very pale yellow colour; and hence, as I am informed, some actually look upon tartaric acid as a

test of the value of commercial iodide of potassium, assuming the salt of which we are now speaking as a standard of comparison. How far it may be depended upon as a test, will appear from what follows.

If tartaric acid in solution be added to a solution of pure iodide of potassium, the commixed solutions are at first colourless, but quickly become slightly yellow, owing to the action of atmospheric oxygen on the hydriodic acid which is thus generated.

On making this experiment with the salt in question, I found, to my great astonishment, that free iodine, in quantity, was instantly developed. I was at first at a loss to account for so great a difference in the behaviour of this salt to that which I had prepared myself, and knew to be pure iodide of potassium; but from the appearance of the crystals of this salt, and from the circumstance of its not being soluble in water to the extent that it should be, I suspected the existence of iodate of potash in it, and I have since convinced myself of the fact of its presence.

I find, if we add tartaric acid solution to a solution of iodate of potash, no change of colour takes place, but that bitartrate of potash is deposited in abundance, and, as a matter of course, iodic acid set at liberty: this solution instantly decomposes iodide of potassium in solution, giving rise to free iodine in great abundance; or, if we add a drop of solution of tartaric acid to a solution of pure iodide of potassium, to which even a minute quantity of iodate of potash has been added, free iodine is instantly developed.

Tartaric acid appears, then, from the experiments I have made, to be a very delicate test of the presence of iodate of potash in iodide of potassium, and will be found a very ready and useful one for this purpose in the hands of the dispensing chemist, showing him that any specimen of this salt in which free iodine is thus developed, is actually of less value than one in which no trace of iodine appears on the instant of its application; inasmuch as iodide of potassium, in a given weight, includes more iodine than iodate of potash does; as is seen at once by the atomic composition of these two salts.

It is well known to every chemist, that one of the methods very commonly resorted to for the production of iodide of potassium is that of acting upon iodine with potash water. In this way we form iodate of potash at the same time; six atoms of potash and six atoms of iodine giving birth to five atoms of iodide of potassium, and one atom of iodate of potash; which latter, if suffered to remain mixed with the iodide, would increase the produce of the manufactured salt nearly five per cent., at the expense of its purity and crystalline beauty.—*Lancet*, Aug. 29, 1840.

ON PEPSIN—THE PRINCIPLE OF DIGESTION.

M. Wasmann has succeeded in isolating pepsin, the peculiar principle of the gastric juice, described by M. Schwann, in the following manner:—

The glandular membrane of the stomach is to be separated without cutting it; it is to be washed and digested in distilled water at

a temperature of 86° to 95° Fahrenheit; after some hours the liquid is to be poured off, the membrane is to be again similarly digested, and to be treated with cold water till it exhales a putrid odour; it is then to be filtered; the filtered liquor is transparent, slightly viscid, and exhibits a remarkable digestive power when a small quantity of hydrochloric acid is added to it. In order to extract the pepsin in a pure state, acetate of lead is to be added to this liquor; the precipitate is washed, diffused in water, and decomposed by a current of hydrosulphuric acid. The filtered liquor is colourless, and has an acid action, owing to the acetic acid.

When this liquor is evaporated at 95° Fahrenheit, to the consistence of a syrup, and absolute alcohol is added to it, an abundant flocculent precipitate is formed, which on drying leaves a yellow gummy matter, which does not attract moisture, and is pure pepsin.

This substance easily dissolves in water, and the solution, even though it contains only 1-5000, dissolves slightly acidulated white of egg, in about six or eight hours. The aqueous solution has an acid action owing to some acetic acid which remains intimately combined with it; it cannot be separated from the pepsinate of lead, even by repeated washings. By ebullition this liquor loses its digestive powers. If the free acid which it contains is cautiously saturated by potash, a small quantity only of which is requisite, flocculi are deposited, and the digestive power is also lost.

The alkalis cautiously added to the solution of pepsin, till the free acid is saturated, occasion the formation of flocculi, and the liquor has no acid action. Sulphuric acid in small quantity produces white flocculi, which redissolve in a slight excess of the acid; by the addition of a further quantity, fresh flocculi are produced; hydrochloric and nitric acids produce the same effects.

Perchloride of mercury occasions a precipitate which is redissolved by an excess of it; the proto- and persulphates of iron and the sulphate of copper precipitate pepsin. Alcohol precipitates it from a concentrated solution. According to M. Pappenheim, this precipitate dissolves in hydrochloric acid, and dissolves boiled white of egg. M. Wasmann confirms this statement of the digestive power of the precipitate formed by alcohol, while, according to M. Schwann, alcohol destroys the digestive property of pepsin.

Pepsin is recognized by the precipitates which its solution gives with diluted acids, and which redissolve in an excess of the acids, and by its giving no precipitate with ferrocyanide of potassium. It is distinguished from albumen by the precipitates which its solution yields on the addition of water and hydrochloric acid; and from caseum, by its acid solutions yielding no precipitate with ferrocyanide of potassium.—*Journal de Chimie Médicale*, Août, 1840.

DECREPITATING SALT OF WIELICZKA. BY H. ROSE.

This salt was first noticed by M. Boué, who sent a specimen of it to M. Dumas; it is distinguished from common salt by decrepitating not only when it is heated, but when dissolved in water; during solution decrepitation occurring with the disengagement of gas. It is evident that this gas was confined in the salt in a state of strong

condensation; and this is the cause which occasions the decrepitation both by heat and solution in water.

M. Dumas found that the gas extricated from this variety of salt, when mixed with oxygen gas, detonated like hydrogen; nevertheless he supposed carbon to exist in it. He had not a sufficient quantity of the salt to examine more minutely the gas condensed in these crystals.

M. H. Rose received from Professor Zeuschner of Krakaut, a considerable quantity of the detonating salt, and he has been enabled to repeat and verify the experiments of M. Dumas. The different portions of salt did not all give the same quantity of gas when dissolved in water. The maximum, as stated by M. Dumas, amounted to about half the volume of the salt.

The gas, when burnt with oxygen, gave nearly the same composition as pond gas (C H^4). This product is probably so condensed as to exist as a liquid or solid in the interior of the salt, and resumes the state of an elastic fluid at common pressures.

The property which this salt possesses ought in future to direct the attention to a great number of minerals which occur in nature, and which decrepitate in the fire without our being able to attribute it to the disengagement of moisture. It may be that the cause of the decrepitation is the disengagement of a gas condensed in the mineral.—*Ann. de Chim. et de Phys.*, Mars, 1840.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1840.

Chiswick.—Aug. 1, 2. Very fine. 3—9. Hot and dry. 10. Very fine. 11. Showery. 12. Cloudy: rain. 13. Cloudy. 14. Rain. 15. Very fine: showery. 16. Fine. 17. Boisterous with heavy rain. 18. Cloudy. 19. Heavy rain: cloudy and fine. 20. Fine. 21. Foggy: very fine. 22. Foggy. 23—26. Very fine. 27. Foggy: fine. 28. Slight fog: rain. 29. Foggy. 30, 31. Cloudy and fine. The mean temperature of the month was nearly 2° above the average.

Boston.—Aug. 1—3. Fine. 4. Cloudy. 5—10. Fine. 11. Rain. 12, 13. Fine. 14. Cloudy. 15. Stormy: rain P.M. 16. Fine. 17. Stormy: rain early A.M.: rain with thunder and lightning P.M. 18. Stormy. 19, 20. Cloudy. 21. Fine: quarter past three P.M. thermometer 80° . 22. Cloudy: rain P.M.: lightning at night. 23, 24. Fine. 25. Fine: rain P.M. 26, 27. Cloudy. 28. Fine. 29. Cloudy. 30. Fine: rain P.M. 31. Cloudy: rain A.M.

N.B. The warmest August since 1826.

Applegarth Manse, Dumfries-shire.—Aug. 1, 2. Very fine. 3. Mild: showery A.M. 4. Fine. 5. Sultry. 6. Sultry: heat oppressive. 7—9. Sultry. 10. Wet and boisterous P.M. 11. Showery. 12—14. Occasional showers. 15. Fair throughout. 16. Much rain P.M. 17. Heavy rain: thunder: high flood. 18. Fine drying day. 19. Fine, with one slight shower. 20. Drizzling all day. 21. Fine: rain P.M. 22, 23. Fine and fair all day. 24, 25. Showery. 26. Fair all day and clear sky. 27. Wet P.M. 28. Fair all day. 29. Drizzling all day. 30. Fine and fair all day. 31. Remarkably fine harvest day.

Sun shone out 27 days. Rain fell 15 days. Thunder 1 day.

Wind north-west 5 days. East-south-east 1 day. South-east $4\frac{1}{2}$ days. South 7 days. South-south-west 4 days. South-west $8\frac{1}{2}$ days. Variable 1 day.

Calm 12 days. Moderate 11 days. Brisk 5 days. Boisterous 3 days.

Mean temperature of the month..... $57^{\circ}60$

Mean temperature of August, 1839 $55^{\circ}70$

Mean temperature of spring water $52^{\circ}33$

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTSON; by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. VALL at Boston, and by Mr. DUNBAR at Applegarth Mense, Dumfriesshire.

Days of Month. 1840. Aug.	Barometer.				Thermometer.				Wind.				Rain.		Dew point. Land: Roy. Soc. 9 a.m.				
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	London: Roy. Soc. 9 a.m.	Self-register. 9 a.m.	Chiswick.		Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Dumfries-shire.	Boston.	Dumfries-shire.					
		Max.	Min.				Max.	Min.											
1.	30.278	30.205	30.133	29.65	30.06	30.00	63.7	72.6	53.2	73	46	64	67	50	NW.	NE.	NW.	SW.	56
2.	30.254	30.171	30.135	29.55	30.00	30.00	66.7	79.0	55.3	83	51	69	66	52	S.	W.	W.	SW.	60
3.	30.244	30.161	30.088	29.53	29.97	30.00	70.0	79.3	61.0	87	48	70	68	53½	S.	NW.	W.	SW.	62
4.	30.188	30.117	30.082	29.50	30.04	30.05	67.5	80.0	61.0	86	56	69	67	55	NW.	E.	calm	SW.	58
5.	30.150	30.081	29.991	29.52	30.06	30.05	67.2	68.5	59.0	82	55	71	75	53	SE.	E.	E.	SE.	61
6.	30.034	29.959	29.917	29.44	30.04	29.98	69.6	73.0	60.0	84	62	68	73	54	E.NE.	E.	E.	SE.	0.03
7.	30.020	29.991	29.930	29.40	30.00	30.03	71.3	78.5	60.0	80	56	70	73	54½	E.	E.	calm	SE. SW	59
8.	30.150	30.129	30.068	29.54	30.12	30.14	65.4	76.0	59.0	80	41	67	73	55½	NNE.	E.	calm	SE. SW	59
9.	30.292	30.223	30.106	29.65	30.18	30.10	60.2	75.5	52.4	83	51	67	73½	54	N.	NE.	calm	S.	54
10.	30.098	30.052	29.779	29.79	29.84	29.49	69.5	77.0	62.0	86	58	65	67½	52	SW.	S.	calm	S.	58
11.	29.614	29.566	29.516	28.94	29.40	29.40	63.5	79.7	62.0	85	55	60	64	52	S.	W.	NW.	S.	60
12.	29.654	29.589	29.555	29.00	29.38	29.30	65.5	70.5	57.0	73	49	51	63	56	WSW.	W.	NW.	S.	57
13.	29.656	29.592	29.551	29.03	29.32	29.35	63.0	70.0	56.0	66	50	55	62	51½	SW.	SW.	calm	SW.	54
14.	29.530	29.672	29.479	28.97	29.31	29.36	61.0	70.0	56.0	66	50	55	62	51½	SW var.	SW.	calm	SW.	54
15.	29.818	29.663	29.752	29.17	29.50	29.66	61.2	67.0	53.5	59	51	60	63½	52	W.	W.	W.	NW.	55
16.	29.968	29.911	29.750	29.35	29.64	29.30	63.0	67.5	54.5	73	54	63	59½	51½	SE var.	W.	SW.	SW.	55
17.	29.206	29.326	29.326	28.70	28.81	28.70	59.8	72.0	58.0	63	49	57	61	50½	SE var.	SW.	NW.	NW.	59
18.	29.374	29.654	29.315	28.66	29.19	29.35	59.5	63.2	52.5	62	50	58½	63½	48	SE.	W.	calm	SSW.	54
19.	29.712	29.799	29.661	29.11	29.52	29.66	57.0	63.8	57.0	70	56	56	64	51½	SE.	W.	calm	SE.	57
20.	30.016	29.957	29.915	29.14	29.69	29.69	60.0	69.0	60.0	79	56	59	64½	53	SE.	S.	calm	S.	56
21.	29.954	29.891	29.775	29.30	29.73	29.68	69.0	70.5	60.0	80	59	71	71½	58	SE.	S.	calm	S.	58
22.	29.836	29.818	29.755	29.40	29.68	29.66	63.5	77.8	62.5	76	50	66	67	50½	SE.	W.	calm	NW.	62
23.	29.998	29.932	29.893	29.30	29.79	29.83	63.0	72.5	56.0	77	46	65	64½	51	WSW.	SW.	calm	NW.	59
24.	30.050	29.976	29.938	29.42	29.78	29.82	62.5	72.5	56.0	77	43	65	63½	47½	SW.	S.	calm	SSW.	57
25.	30.038	29.968	29.907	29.38	29.74	29.75	63.5	73.5	54.4	72	58	63	60	48	WSW.	SW.	calm	SSW.	55
26.	30.060	30.001	29.964	29.42	29.88	29.88	65.0	73.0	61.0	72	49	62	64	42	NW.	S.	calm	S.	61
27.	30.110	30.011	29.936	29.41	29.80	29.80	61.5	70.8	56.7	73	55	66	65½	53	W.	W.	calm	var.	59
28.	30.060	30.044	29.991	29.33	29.92	30.00	63.5	72.0	60.0	74	55	68	61	54	W.	W.	calm	var.	61
29.	30.204	30.140	30.074	29.57	30.01	29.95	60.9	72.0	60.0	73	57	64½	63½	54	E.	E.	calm	var.	60
30.	30.128	30.053	29.920	29.45	30.02	30.15	66.8	69.0	58.0	82	58	69	63½	54½	E.	E.	calm	SSW.	62
31.	30.184	30.116	30.040	29.60	30.18	30.15	63.0	79.5	60.0	74	58	59	64	41½	NE.	NE.	calm	ESE.	59
Mean.	29.964	29.928	29.841	29.31	30.406	29.757	64.3	72.7	57.9	76.00	52.68	64.2	61.7	51.5					Mean.
																			58
																			1.272
																			1.62
																			1.22
																			3.69
																			Sum.
																			1.272

THE
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[THIRD SERIES.]

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XLVI. *On the mean Level of the Sea.* By the Rev. W. WHEWELL. F.R.S., Professor of Moral Philosophy in the University of Cambridge*.

IN the Philosophical Magazine for August last, page 134, and the following, are some remarks by Mr. Richard Thomas, on the Account of a Level Line, published in the Transactions of the British Association for 1838. The level line was carried from the Bristol Channel to the English Channel by Mr. Bunt, under my direction: the conclusion which I drew from the operation was, that the *mean* height of the sea was at the same level at the two extremities of the line; and this conclusion Mr. Thomas disputes. I beg to offer a few observations on the subject, suggested by his remarks.

In the first place, I will make one or two observations on the theoretical view of the subject. It is certain (although this view does not appear to be familiar to most of those who turn their thoughts to the question) that according to theory, *the mean surface of the sea* (in open water) is *invariable* in its height; and that however different may be the level of high water, or of low water at different places, the line of mean water is a level line. This will appear from the following considerations.

The time of high water is different at different places, and thus there are, at any given moment, unequal pressures operating upon different portions of the sea. It is high water at the Land's-end when it is low water at Beachy Head; and therefore the fluid pressures in different parts of the English Chan-

* Communicated by the Author.

nel are not in equilibrium at any moment. And this is quite consistent with mechanical principles; for the difference of pressures in different parts of the channel is precisely the force by which the tide-wave is transferred along the channel. These oscillations of the pressure and of the surface, correspond to each other; being respectively the changes of force and the motion produced. But the deviations above and below the mean pressure and the mean surface, balance each other in the course of a few hours. If the mean surface were not a level surface, the mean pressures would not be in equilibrium, and there would be in one part of the fluid a *permanent* resulting excess of mean pressure; which excess would tend to carry the fluid from the place where its mean level was highest, and to restore the mean surface to a level, just as if the fluid were at rest.

This being understood, it will not be difficult to see what will be the state of the levels of high, low and mean water under any supposed circumstances of a port.

If the water be deep enough, and the surface of low water not much contracted, the motions of the high-water surface and of the low-water surface will be nearly equal, and the mean water will be at the constant height of the level surface: this is the case at Liverpool, as I have shown from Captain Denham's observations in my Twelfth Memoir on the Tides (in the Phil. Trans. 1840).

If the surface of low water be much contracted, the volume of water subtracted from the mean state of the sea in order to make low water, will (in the interior of the port) be nearly equal to the volume added, in order to make high water; and as the horizontal surface is smaller, the depth will be greater: this is the case at Plymouth; where, as I have shown in my Tenth Memoir, the motions of the low-water surface are greater than those of the high-water surface.

If the inlet be a river, in which, by various causes, the tide-wave is gradually extinguished in its progress upwards, and in which, without the tide, the water would flow towards the sea, the high water will be most nearly a level surface, and the mean water line will slope towards the sea. This is the case in the river Thames, in which, as appears by the researches of Messrs. Rennie, the surface of high water is nearly a level surface, while the low water surface slopes considerably.

Finally, if in consequence of shallowness, or a bar, the water cannot run out to the level of low water in the sea, the height of apparent low water will be constant, and the apparent mean water will be charged with half the changes of the high water

Bearing in mind these results, it will be easy to reply

to Mr. Thomas's remarks. Mr. Thomas thinks that the sea is probably kept above its average height at Axmouth, one of our stations, by the projection of Cape la Hogue into the English Channel. But from what has been said, it must be plain that this circumstance cannot alter the mean height of the sea on either side of this supposed barrier. A channel fifty miles wide, between Cape la Hogue and the Bill of Portland, is certainly enough to allow the water to assume its mean level, as determined by the mean forces; especially when we consider that it has had an indefinite time allowed to do this in. Indeed, we may satisfy ourselves that this contraction of the surface is no valid reason for the mean surface being higher on the eastern side, if we recollect that it is just as valid a reason for the surface being lower on that side; since the contraction prevents the water finding its level one way as much as the other.

It is true that a contraction of a channel in some cases elevates *high water*; but this is precisely because high water is a case of fluid *motion*, not of fluid *equilibrium*. We may apply to such cases the principle of the conservation of *vis viva*. Each particle of fluid can temporarily ascend to the height due to its velocity; and hence an increased velocity, arising from a contracted channel, makes an increased temporary elevation possible. This consideration also explains the rapid changes in the amount of tide, which take place as we proceed from one part of the sea to another. And hence we see how little we are justified in drawing any inference concerning levels, from any facts of high or low water. The total tide on the coast of Wexford and Wicklow is not more than three feet; on the opposite coast of Wales, (the sea being very narrow,) it is between 20 and 30 feet. If the surface of low water were a level surface, as Mr. Thomas supposes, we should have the mean surface of one side of this narrow sea *constantly* 10 feet higher than the mean surface of the other side; a result quite impossible: whereas a *temporary* elevation or depression in one part of a fluid surface seven or eight times as great as in another part, is quite familiar to us, and offers no difficulty.

In order to show that the mean surface of the sea is not constant, Mr. Thomas refers to observations of his own, made at Falmouth, Bristol, and other places. Upon these I would beg to remark, that he appears to me to consider principally detached observations; whereas we cannot hope to arrive at any satisfactory result, in any other way than by taking the *means* of *series* of observations. We know well that special causes, some of them known and some yet unreduced to rule, perpetually affect particular high waters and low waters; and

therefore affect the mean waters as deduced from these. Thus, for instance, the diurnal inequality at Bristol might make two successive high waters differ by four feet; and thus would make the height of mean water differ by two feet, according as the one or the other of these high waters was compared with the intermediate low water. Add to such inequalities those arising from the varying pressure of the atmosphere, the wind, and the like, and we may easily conceive the possibility of such deviations from constancy in the half-tide mark as Mr. Thomas records. Yet these deviations do not prevent the mean of any fortnight's observations from being constant within a very few inches; as it will be found to be at Liverpool, Plymouth, and any other place similarly circumstanced.

It will be observed, that I allow that in a *river* the line of mean water slopes towards the sea; but this condition does not affect either end of our level line; and therefore I consider that the result which we obtained, namely, that the height of mean water is the same at its two extremities, represents the general condition of the ocean.

Mr. Thomas justly remarks, that we have no authority, in our operations, for placing the mean water at Plymouth and at Axmouth at the same level, since our leveling did not extend to Plymouth. Accordingly, we have not drawn any inference from this supposed identity of mean level. The Plymouth observations were referred to, as is stated in the Report, merely in order to ascertain that there were no unestimated inequalities in the tides on that coast at the time of our observations. But if *we* have as yet no authority, except that of theory, for assuming the mean waters at Plymouth and Axmouth to coincide, Mr. Thomas has no authority, either from theory or observation, for assuming the high waters to coincide. And all his other inferences from high waters and from low waters, respecting the level of the sea, are equally gratuitous.

For the reasons stated here and in the Report, I consider it as certain, that on all open coasts the heights of mean water form a level surface. But I entirely agree with Mr. Thomas, that an extension of this leveling operation to other points of the coast would be desirable. In the conclusion of my Report, I have stated several other advantages, besides the confirmation of the above doctrine, which would be placed within our reach by such an extended system of levels. It must, however, be obvious to every one, that such an operation, conducted with that care and exactness which alone could render it valuable, would be business of very serious trouble and expense. To have afforded one good example of such an opera-

tion is, I conceive, a merit which gives the British Association a just claim to the gratitude of the scientific world.

Trin. Coll., Cambridge, Oct. 2, 1840.

XLVII. *Observations on certain Peculiarities of form in the Blood Corpuscles of the Mammiferous Animals.* By GEORGE GULLIVER, F.R.S., F.Z.S., Assistant Surgeon to the Royal Regiment of Horse Guards*.

IN the course of an extensive series of observations†, I have had occasion to remark, that the red particles of the mammiferous animals are singularly susceptible of change in size and form, as if from the effect of organic contractility‡. My attention had long been directed to this subject, when it became still more interesting to me from the results of the examination of the blood of certain deer. A notice of the singular appearances assumed by the blood corpuscles of these animals was given in the Dublin Medical Press of December 18, 1839, and in the Philosophical Magazine for January, 1840; and more fully described, with an illustrative drawing, in a paper read before the Royal Society in February, 1840, and which forms part of the present communication.

In that paper I expressed a doubt whether the remarkable forms presented by the blood particles might not have resulted from changes in the ordinary blood discs; for it was observed that the peculiar figures were more numerous after the blood had been allowed to stand for a short time, while they all disappeared and returned to the circular form when treated with a small quantity of water. In continuing my observations I soon had further reason to confirm this view, as I mentioned in a note on the subject written in the beginning of last March to Dr. Davy. In some specimens of blood which I had recently obtained from the Mexican, Porcine, and Persian Deer, the peculiar corpuscles were by no means so numerous as on former occasions, and in some of the trials were seen but very sparingly, while in other instances the singular particles were present as abundantly as ever.

Hence it became important to ascertain the conditions

* Communicated by the Author.

† Lond. and Edinb. Phil. Mag. for Dec. 1839, and in the Numbers for January, February, March, and August, 1840.

‡ Mr. Ancell, in his excellent "Lectures on the Blood and other Animal Fluids," mentions that Schultz describes the blood corpuscles as possessed of very remarkable organic contractility. See *Lancet*, 1839, p. 147 and 386. Eben and Mayer regarded the red particles as infusory animals.

I take this opportunity of expressing a hope that we may soon have an English version of the interesting papers entitled, "*Untersuchungen über die Structur und Veränderungen der Chylus- Lymph- und Blutkörperchen*," by Dr. Herman Nasse, and published in the valuable "*Untersuchungen zur Physiologie und Pathologie*;" by Drs. Friedrich and Herman Nasse, zweiter Band, Heft 1 and 2.

under which these particles were produced; and although my researches have not hitherto proved perfectly satisfactory in this respect, yet it appears to me that they afford evidence that the blood corpuscles have a contractility or irritability inherent in themselves, which may continue some time after their removal from the body, and enable them to assume permanently the very anomalous forms which I have described and figured. In some instances a large number of the red granulated particles appeared to be produced by the irregular shrinking of the smooth discs*, in others there was a manifest contraction of the particles while under examination in their own serum†, and the corpuscles were very quickly and remarkably modified in figure when extravasated and subjected to contact with the neighbouring tissues‡, evincing the readiness with which the blood discs assume new forms, perhaps connected with the plastic force. It has appeared to me also, that in some cases modifications in the shape of the blood discs might be attributable in great measure to violent action of the heart produced by the fright and struggles of the animal; for when the blood was obtained without at all alarming him the irregular corpuscles were not numerous, while these particles were seen abundantly in blood which was taken when the deer was confined, and made every effort to struggle and get loose, so that the circulation became greatly accelerated. In the Muntjac, a very shy and wild animal, the peculiar particles were always seen in great numbers; but from the more docile Mexican, Porcine, and Persian Deer, a drop of blood could occasionally be obtained before the animal was aware of the operation, and in such cases the irregular shapes were observed but sparingly.

However, although I am disposed to think that in some ruminants, particularly in those just mentioned, the peculiarities in the particles were more or less influenced by the state of the circulation; yet, from the result of a few trials on other animals, it did not appear that fright or violent muscular exertion was always capable of producing the singular forms in question, although it seems to me that the previous observations render it worthy of inquiry, how far the blood discs may be affected by the state of the circulation, or their contractility influenced through the medium of the nervous system. That the blood particles do contract and assume various forms, is certain; but whether this be a vital or merely a physical phænomenon, is a subject of much interest, and well deserving of new researches. There appears to me to be some good grounds for believing that the blood corpuscles do possess an inherent power of contractility.

* See Lond. and Edinb. Phil. Mag. for February, 1840.

† *Ibid.*

‡ *Ibid.*

The following paper, already referred to, contains a description and figure of the peculiar corpuscles before mentioned. I have since seen similar forms in the blood of some other animals; and recently in one of the carnivora (*Genetta tygrina*) I observed the crescentic, spear-shaped, and sigmoid particles in great numbers. The animal was so difficult to secure, that its circulation must have been much excited before the blood was obtained. In a second trial, when the blood of this genet was examined immediately, the peculiar corpuscles were not abundant, although in the course of a few hours there were scarcely any other particles to be observed, and the forms shown in the wood-cut remained until putrefaction began.

It will be seen from the figure, that the appearance of many of these corpuscles is very similar to that of the elongated cells represented by Schwann as concerned in the growth of certain tissues. Dr. Martin Barry has lately noticed that the blood corpuscles in certain cases undergo rapid changes; and he announces that the muscular tissue, and the cells of the chorion, are formed from the corpuscles of the blood*. The pus globules, which have recently been regarded as organic cells, have been frequently considered in this country as transformed blood discs, and on the continent M. Gendrin announced that he had actually seen the blood corpuscle changed into the pus globule†.

Observations on the Blood Corpuscles of certain Species of the Genus Cervus. By GEORGE GULLIVER, F.R.S., Assistant Surgeon to the Royal Regiment of Horse Guards.

(Read before the Royal Society, Feb. 6, 1840.)

The blood corpuscles hitherto described in the vertebrate animals, have either a circular or an elliptical form. Till the late discovery by M. Mandl, of the latter shape in the particles of the Dromedary and Alpaca, and my more recent observation of the same form in the Vicugna and Guanaco‡, the blood discs were supposed to be circular in all the mammalia, and the oval corpuscles to be confined to the lower divisions of the vertebrate animals.

I have now to describe some peculiar forms of the blood corpuscle, which I believe have not hitherto been observed

* Proc. Royal Society, May 7, 1840.

† See my Researches on Suppuration, Phil. Mag., Sept. 1838; on the Softening of Fibrine, Med. Ch. Trans. v. xxii.; on Pus, London Medical Gazette 1839—1840, pp. 201 and 415.

‡ See Dublin Medical Press, Nov. 27, 1839. My paper on the Blood and Pus particles of the *Camelidæ*, just published in the twenty-third volume of the "Transactions of the Royal Medical and Chirurgical Society," was read before that Society, Nov. 26, 1839.

in any class of animals. These corpuscles I have examined particularly in the Muntjac, Porcine, and Mexican Deer.

I observed in the blood of these animals a large quantity of crescentic or lunated particles, besides a few of the common circular figure. The former are very remarkable from their great number and distinct shape; they are acutely pointed at the ends, gibbous in the middle, with a convex and concave margin; or being without the concavity, they merely present the figure of the segment of a circle.

But there are other forms equally singular: frequently they are not curved, but straight, and gibbous at the sides—lanceolate, to use a botanical term; occasionally they are obtuse at one end, something like a comma in shape; or, from an acute projection of the convex part, approaching to a triangular figure. I have seen them also nearly square, and not uncommonly with elongation of the angles and concavity of the margins, the latter peculiarities being also sometimes observable in the triangular particles. Finally, they may present a sigmoid figure, as if from twisting of the ends of the lanceolate forms.

Like the common blood discs, these peculiar corpuscles are easily deprived of their colouring matter and rendered invisible by water; but if only a very small quantity of this fluid be added to them, they quickly swell out and assume an oval or circular figure, forming, by the approximation of their edges, long bead-like strings. When treated with saline solutions, the oblong particles become rather smaller, but preserve their figure tolerably well.

These singular particles constitute the greater number of those present in the serum, particularly if the blood be kept three or four hours after having been taken from the animal; and the forms are often well preserved in cool weather for nearly a week; in perfectly recent blood they seem to be rather less numerous, although in a short time all the shapes may be recognized abundantly*.

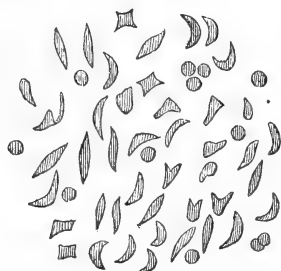
If these singular corpuscles are merely the result of changes of form in the circular discs, such transformations would appear to be altogether peculiar, and at variance with all our previous knowledge of the blood corpuscles of the mammalia.

The first impression will probably be that some of the forms I have described are those which may be presented by different views of the circular discs as they revolve on their axes. Hence it may be necessary to observe, that the crescentic corpuscles are sometimes seen to turn over in the field

* The blood was constantly examined as soon as possible after it was taken from the animal—often within twenty minutes, and always in less than an hour; and in every case directly the blood flowed through the wound specimens were dried instantaneously on glass, with the precautions mentioned in my paper in the *Phil. Mag.* for Feb. 1840.

of vision, and that the lanceolate particles often so revolve. Besides, their length much exceeds the diameter of the circular discs; the extremities of the oblong corpuscles are acutely pointed, and their form and appearance is altogether remarkably distinct and peculiar. Finally, I have preserved dried specimens, which I shall be happy to show to any gentleman who may feel an interest in the subject; and as the animals are alive in the collection of the Zoological Society, no difficulty is likely to remain in the way of inquiry into the facts recorded in this paper.

For the accompanying drawing (see wood-cut) I am indebted to my friend Mr. John Dalrymple, who executed it from two portions of blood which I sent to him for the purpose; one being dried on glass, and the other preserved in a weak solution of common salt. The larger group represents the corpuscles as he saw them with a deep achromatic object-glass adapted to a compound microscope. The smaller group exhibits the most remarkable forms of the corpuscles selected and compared together.



The following observations were made at the time the blood was examined.

1. Reeves's Muntjac Deer (*Cervus Reevesii*). The circular discs 1-7200th to 1-6000th of an inch in diameter; the oblong particles from 1-4000th to 1-2666th of an inch in length, and 1-12,000th to 1-8000th in breadth, at the gibbous part. Examined in the recent state, also in urine, in a weak solution of muriate of soda, and dried on glass. In the fresh specimens, the spear-shaped, lunated, and common circular corpuscles often seen turning over in the field of vision. There appeared to be a very great variety in the form of the corpuscles; for besides those already described, there were many oval and egg-shaped, and even triangular or square particles, the two latter indeed not so accurately defined as the former. All these figures may be seen in the corpuscles of the Mexi-

can and Hog Deer, but not so numerous as in this specimen of blood from the Muntjac. It was obtained from a vein of the ear; the animal a male, nearly full-grown, and bred in the Zoological Gardens, where its parents died, and are preserved in the museum of the Society.

2. Mexican Deer (*Cervus Mexicanus*). Circular corpuscles not so numerous as some very singular oblong particles, similar to those seen in the blood of the Muntjac. The circular corpuscles generally about 1-6000th of an inch in diameter; the oblong corpuscles 1-3200th to 1-2400th of an inch in length, and 1-12,000th to 1-8000th in breadth, at their gibbous part. In this situation a few of them are as large as the circular discs. The blood examined was obtained from a vein of the ear of a female, apparently full-grown. The observation was the same whether made on the recent or dried blood. The veins in the ear of the animal are numerous and very apparent. From another bleeding, a week after the first, the same result obtained.

3. Hog Deer (*Cervus porcinus*, albino var.). In some blood obtained from a prick of the upper lip, the corpuscles were found to be similar in size and shape to those described in the Muntjac and Mexican Deer, except that some of the circular discs appeared a little larger, and they were generally as numerous as the singular oblong corpuscles. For a second trial, some blood was obtained from an incision of the ear, about a fortnight after the first bleeding, and the former result confirmed. The animal was a male, apparently full-grown.

In conclusion, it may be remarked that the peculiar particles now described, so singularly variable and anomalous, and susceptible of such remarkable mutations in their figure, afford a very interesting subject for further inquiry, which may probably illustrate the physiology of the blood corpuscles. At present so little is known respecting these curious bodies, and so much is to be expected from future research, that we know not what degree of importance may belong to them, although there is reason to believe that the discovery of their use and method of formation would be of great value to science.

An Appendix to the "Observations on the Blood Corpuscles of certain Species of the Genus Cervus." By GEORGE GULLIVER, F.R.S.

(Read before the Royal Society, Feb. 6, 1840.)

Since I had the honour of transmitting my communication to the Royal Society "on the Blood Corpuscles of certain species of the genus *Cervus*," I have had an opportunity of examining some blood obtained from a deer lately received at the Zoological Gardens from the Persian mountains. I am

not aware that the species of this animal has yet been determined, or indeed whether it has ever been described. It is about as large as a full-grown fallow deer.

The blood was procured from a puncture in the upper lip of a female, under circumstances very favourable for the preservation of the size and figure of the corpuscles; they were quickly dried on slips of glass, and submitted to observation with as little delay as possible.

The greater part of the blood corpuscles presented the usual circular shape, although often irregularly triangular, quadrangular, or polygonal; and there were many of the singular forms described in the Muntjac, Porcine, and Mexican Deer. The latter particles corresponded very nearly in size with the measurements already given; the former were rather larger, having an average diameter of $\frac{1}{50666}$ th of an inch, the small and large discs measuring respectively $\frac{1}{60000}$ th and $\frac{1}{40000}$ th of an inch.

Although I would not venture to communicate to the Society any account of the blood corpuscles from a single observation, yet it appears to me that the present one may be considered as an addition to those which I have already made, with as much care as I could, concerning the blood of the other species of deer.

Whether the peculiar corpuscles exist in the circulation of the animals, or may be the effect merely of changes in the form of the circular discs immediately after their abstraction from the vessels, is a subject for further and special inquiry, and one which may tend to throw some light on the nature of the blood corpuscles. But whatever may be the result, the facts will hardly be less singular and remarkable, and I am not aware that they have hitherto attracted the attention of physiologists, notwithstanding the minute examination to which the blood particles of different animals have lately been subjected.

XLVIII. *On some Combinations of Arsenic with Cobalt.* By THEODORE SCHEERER, and WILLIAM FRANCIS*.

THE combinations which we have to describe in the present paper, were formed in the course of a smelting process which was introduced some years since by Inspector Roscher, into the smalt-works at Modum in Norway. The object of this process is to deprive the prepared and roasted cobalt ores of a great part of their iron and arsenic by smelting, in order to obtain higher and purer kinds of smalt from them.

First Combination.—This forms long fascicular conglo-

* Communicated from Berlin by the Authors.

merated crystals, terminated at the extremities by an oblique plane. Its dull surface, arising from a film of oxide, together with the fascicular arrangement, does not allow of a more accurate crystallographical determination; it is probable that they belong to the mono-dimetric system. Some of the crystals, or rather of the bundles of crystals, were above two inches long, and about two lines in thickness. The analysis was performed in the mode usually employed for the arsenic metals. Cobalt and iron were separated from each other by approximate neutralization of the sulphate solutions of the two metals, by dilution and then boiling them.

The result of the analysis was

Sulphur	0·16
Arsenic.....	36·02
Cobalt	53·71
Iron.....	10·05
Copper	0·86—100·80.

The peroxide of iron here obtained was found before the blow-pipe to be perfectly free from oxide of cobalt; the oxide of cobalt, however, contained a slight trace of iron, which was separated by dissolving the oxide in hydrochloric acid, and precipitating with an excess of ammonia. It may have amounted, at the utmost, to a few milligrammes.

To be certain that these crystals contained no admixture of foreign substances, but were a pure chemical combination, the following experiments were made with other portions of them. In the first place, the amount of arsenic, the accurate determination of which was most essential, was ascertained in two other additional cases, and found to be as follows:—

1st, 36·41 per cent.

2nd, 35·34 —

approximating, therefore, to the quantity first found, 36·02.

The sum of the oxide of cobalt and peroxide of iron, which in the first analysis had amounted to 2·061 grammes, was, in a second examination, for which a quantity differing from the first only by 0·005 was employed, equal to 2·067 grammes. In both experiments the oxides were kept at a red heat in an open platinum crucible, until they no longer increased in weight*. Lastly, the quantity of cobalt was again determined by a second trial, and found to be 54·90; whereas the first gave it 53·71. The differences occurring between these several results are certainly not such as to give rise to

* When a larger quantity of the peroxide of iron is heated with a smaller quantity of the oxide of cobalt for a short time in a closed platina crucible, the latter scarcely becomes more highly oxidized, and in analyses not requiring extreme accuracy it may be admitted that no superoxide is formed.

any doubt as to the homogeneity and purity of this combination. If, therefore, it be admitted that the above-mentioned quantitative composition affords a perfect symbol of the nature of the combination, then the atomic relation of arsenic to the other metals would be as the numbers

$$7.74 : 17.76,$$

that is, if it be assumed that the very slight quantity of sulphur occupies the place of a portion of the arsenic, and that copper, cobalt and iron are here isomorphous. That this actually is the case, will very clearly appear from the experiments subsequently detailed.

Second Combination.—This is crystallized in large laminæ of metallic lustre, which for the greater part are so rooted in the mass, that it was not possible to determine from the termination of the laminæ the crystallographic system to which they belong. Frequently several such laminæ are superposed one upon the other, and project with dull superficies beyond the surface of the rest of the metal. They have then an appearance quite similar to that of the above-described combination. The analysis was performed differently from the former, inasmuch as the cobalt and iron were separated from each other by the succinate of ammonia.

The result was as follows:—

Sulphur	0.50
Arsenic	35.20
Cobalt	31.35
Iron	23.15
Copper	8.90—99.10.

The peroxide of iron separated by succinate of ammonia evinced, indeed, before the blow-pipe the action of cobalt, but in so slight a degree that it seemed of no importance. The oxide of cobalt, however, on being dissolved in muriatic acid, and treated with ammonia in excess, left behind 0.033 gram. of the peroxide of iron; the quantity of which, however, was of course increased by some oxide of cobalt precipitated with it*.

* More accurate results, as to the application of succinate of ammonia for the separation of cobalt from iron, are, as far as my experience goes, extremely difficult to obtain. If it be attempted more perfectly to neutralize the fluid previously to the addition of the succinate of ammonia, the peroxide of iron may very likely prove to contain somewhat too much cobalt. The method that I have recommended for the separation of these two metals, it is true, has also its difficulties; however, in most cases I have had better success with it, and scarcely ever worse, than with succinate of ammonia. At the same time this very great advantage should be considered, that the solution containing cobalt, filtered from the iron, can be immediately thrown down with potash; while, in the separation by

If we calculate the atomic relation for this combination in the same manner as for the former, we obtain

$$7.74 : 17.74,$$

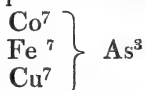
therefore exactly coinciding with that of the first combination. From the above-mentioned composition of the latter, it appeared probable that the small quantity of sulphur in it was combined with the likewise only small quantity of copper forming the sulphuret of copper; and that this was merely mechanically mixed with the remainder of the combination. This seemed to be the more admissible, as small quantities of the sulphuret of copper often collect as slag (*speise*) on the floor of the smalt-ovens at Modum*. The analysis of the second combination proves, however, that copper and sulphur do not stand in any relation, for here the quantity of the first is increased to 8.90 per cent., while that of the latter amounts merely to 0.50 per cent.

With respect to the formula for the two combinations, it must turn out to be the same for both, as the atomic relations

$$7.74 : 17.76$$

and $7.74 : 17.74$

approach so very near to each other. It is almost perfectly coincident with the composition ascertained, if we adopt it at



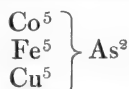
means of succinate of ammonia, the oxide of cobalt is obtained dissolved in an ammoniacal fluid, from which it has to be precipitated by sulphuret of ammonium. The filtration of the sulphuret of cobalt is, however, as is well known, one of the least pleasant operations of analytic chemistry. That the separation by my method sometimes proves unsuccessful, arises, perhaps, not so much from the point of neutralization not being closely enough attained, but rather, 1st, from the dilution after saturation not being carried far enough; or, 2ndly, from the solution not being sufficiently stirred during saturation upon the addition of each fresh portion of caustic potash; or, 3rdly, from a potash being employed containing carbonic acid, thus causing the fluid (from the carbonic acid dissolved therein) to act acidly, even when the point of neutralization has been already exceeded. Care must also be taken, that on the stirring round, no flocks of the oxide of cobalt, which are precipitated at the first moment upon the addition of the caustic potash, but soon redissolved, adhere to the margin of the porcelain basin, and, drying upon it, escape solution. These, indeed, are all circumstances which must be attended to, and which occasion some difficulty in the employment of my method; but which are easily overcome by some practice. Whenever we shall be able to determine in analytic chemistry all bodies as easily and strictly as barytes and sulphuric acid, then, indeed, such methods of separation as that just described will become superfluous.—SCH.

* Nickel slag (*Nickelspeise*) does not occur here at all.—SCH.

in which case the atomic relation should be as

$$7.74 : 18.06.$$

Such a numerical relation appears at first sight, it is true, somewhat unusual. The formula

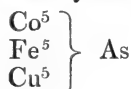


would, perhaps, be more probable; however, the atomic relation corresponding to it, viz.

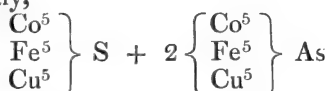
$$7.74 : 19.35$$

differs still more from the ascertained result.

There sometimes occurs a third kind of crystallized arsenical cobalt in the smelting processes at Modum. It forms dendrites of a kind quite similar to those of speiss-cobalt, or such as are obtained as pure copper in the [*aussaigerung*] of gun-metal*. This, therefore, would prove that its crystalline form would belong to the regular system. An analysis which was made about seven years since gave approximately the formula



or, more correctly,



since beside 14 per cent. of arsenic, it contained 3 per cent. of sulphur. The copper amounted to 7.86 per cent. However, the iron and cobalt were separated in a defective manner, whence the former contained a considerable amount of cobalt. The analysis needs therefore repetition; although, since iron and cobalt do not differ much in their atomic values, scarcely any essential change in the formula might ensue.

XLIX. Examination of a crystallized Nickel Ore. By

WILLIAM FRANCIS, A.L.S.†

THE combination which is the subject of the present paper, although consisting mainly of arsenic and nickel, yet has to be distinguished from the kind of nickel ore analysed some time since by Professor Wöhler. Whilst the latter is crystal-

* I obtained of Dr. Marchand, who is at present engaged in very interesting experiments on alloys for fire arms, a piece of dendritic copper bearing very great resemblance to the native.—SCH.

† Communicated from Berlin by the Author.

lized in octahedrons, and is deposited from the smalt furnaces; the former is crystallized in large laminæ, very similar to the second cobalt ore described in the preceding Article, and is evidently the product of a further process of purification: nor was it obtained directly from a smalt-work, but from the German silver manufactory of Mr. Henniger in Berlin. This process of purification seems to resemble that which is practised at Modum with the cobalt ores.

The analysis was performed in the laboratory of Professor Henry Rose; and the course pursued quite similar to that employed for combinations of arsenic. The nickel and iron, however, were not separated by succinate of ammonia, but determined according to Dr. Scheerer's method. The result of the analysis was

Sulphur	1.01
Arsenic	34.07
Nickel	52.58
Cobalt	3.28
Iron	10.06—101.00.

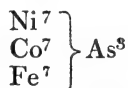
The peroxide of iron obtained was again dissolved in hydrochloric acid, and precipitated by ammonia in excess. Sulphuret of ammonium produced no perceptible alteration in the filtered ammoniacal fluid. From the obtained oxide of nickel, however, 0.009 grammes of nickeliferous peroxide of iron were separated by solution and the addition of ammonia: which quantity had not been separated by the above method.

In this separation, therefore, Dr. Scheerer's method gave a better result than that with succinate of ammonia in the analysis of the cobalt ore described in the preceding Article.

If it be admitted that the slight quantity of sulphur takes the place of a part of the arsenic, the atomic weight of the arsenic is to that of the other metals as

$$7.74 : 18.10,$$

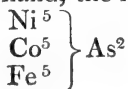
therefore, exactly as in the combinations detailed in the preceding Article. The formula for this nickel ore would accordingly likewise be



In this case the atomic proportion should be

$$7.74 : 18.06,$$

coinciding, therefore, very nearly with that given by the analysis. If, on the other hand, the formula



were adopted, the proportion

7·74 : 19·35

would differ more from the result of the analysis. According to the first supposition, the combination, in 100 parts, should consist, admitting only nickel and arsenic as essential constituents, of

Arsenic 35·25

Nickel..... 64·75

and with this the analysis agrees exceedingly well. According to the second formula ($\text{Ni}^5 \text{As}^2$), on the contrary, the composition should be

Arsenic 33·69

Nickel 66·31.

Now, whichever formula may be considered as most correct, thus much is certain, that the combinations described in the preceding Paper, and in this, are by no means composed according to simple relations, as 1 : 2, or 2 : 3; nor will this appear at all extraordinary, in a metal such as arsenic, which forms an acid consisting of two atoms of radical and five atoms of oxygen. It is, indeed, questionable whether the formula $\text{Ni}^3 \text{As}^3$, laid claim to by Prof. Wöhler for the nickel ore analysed by him, does not likewise possess an atomic relation similar to that of the combinations here treated of.

Now the proportion 2 : 3, 3 : 5 approach one another very nearly, as the following calculation shows :—

	$\text{Ni}^3 \text{As}^3$	$\text{Ni}^5 \text{As}^3$
Nickel	54·15	56·75
Arsenic ...	45·85	43·25.

Professor Wöhler, however, found the composition to be

Nickel 55

Arsenic 44,

approaching, therefore, nearer to the formula $\text{Ni}^5 \text{As}^3$ than to $\text{Ni}^3 \text{As}^2$. It is therefore very probable that the number 5 also occurs in the atomic relation of the constituents in this combination; and there would then have been found on the whole the following proportions between cobalt (or nickel) and arsenic :

Third cobalt-combination $\text{Co}^5 \text{As} (?)$

First and second do. as like- $\left\{ \begin{smallmatrix} \text{Co}^5 \\ \text{Ni}^5 \end{smallmatrix} \right\} \text{As}^3$, or, $\left\{ \begin{smallmatrix} \text{Co}^7 \\ \text{Ni}^7 \end{smallmatrix} \right\} \text{As}^3$
wise purified nickel ore

Common nickel ore $\text{Ni}^5 \text{As}^3$.

The middle combination will, according to theory, be regarded as far more probably $\text{Co}^5 \text{As}^2$; although the results of the analyses bring it nearer to the formula $\text{Co}^7 \text{As}^3$.

L. *On the Variation of the Semi-axis Major of the Moon's Orbit.*
By JOHN WILLIAM LUBBOCK, Esq., Treas. R.S. F.R.A.S. and
F.L.S., Vice-Chancellor of the University of London, &c.

POISSON, in his *Mémoire sur le Mouvement de la Lune autour de la Terre*, has considered the following theorem, that the expression for the variation of the semi-axis major contains no argument of long period, accompanied by a multiple of m less than m^4 . In this paper he has taken into account the terms which arise from the second approximation, or that in which the squares and products of the quantities $\delta \xi$, δa , δe , $\delta \omega$, $\delta \gamma$ and $\delta \alpha$ may be neglected. It is evident that terms may arise in the next, and indeed in every succeeding approximation of the order m^3 , which must be taken into account in order to prove the proposition with sufficient generality. Thus the variation of the eccentricity contains terms multiplied by m , as of the argument $9, (2\tau - 2\xi)$; these, multiplied by others of the order m^2 , give in δe^2 terms of the order m^3 , and these multiplied by $\frac{d^3 R}{d e^2 d c}$ give in $\frac{d R}{d c} dt$ terms multiplied by m^5 , which after integration become of the order m^3 ; if the argument being of the kind under consideration, the divisor introduced by integration is of the order m^2 . It is true that Poisson refers to his *Mémoire sur la Variation des Constants arbitraires* *Mém. de l'Académie*, tom. i., for an extension to the third approximation, that is, to terms depending upon the cube of the disturbing force. In this paper, however, expressions are employed, which take for granted that the disturbing force can be exhibited in the same form, developed in terms of the initial values of the coordinates x, y, z , and of the initial values of their first differential coefficients $\frac{dx}{dt}, \frac{dy}{dt}, \frac{dz}{dt}$. This development has never been executed, nor has it been shown to be possible. When this system of constants is employed, the quantities which are equivalent to $[a, \omega]$, $[a, e]$, &c., become equal to unity and rigorously constant, so that it is unnecessary to consider the effect produced by their variation. Even, however, with the simplifications which recourse to this peculiar system of constants affords, Poisson admits that this demonstration would become too complicated to admit of its extension to the higher powers of the disturbing force. M. de Pontécoulant has made objections to the proof given by Poisson, but as he differentiates* in a manner at variance with that intended in the expressions of which Poisson makes use, and does not allude to the Memoir in question, in which a further approximation is attempted, these objections are not to be considered as exactly identical with those of that distinguished astronomer.

* *Conn. des Temps*, 1840, p. 21.

In the Lunar Theory terms of the order m^3 , and of the nature of those under consideration, may arise not only in the third but in every succeeding approximation, and hence it becomes absolutely necessary to seek some mode of proof which admits of unlimited extension, and I have therefore endeavoured to modify the proof given by Poisson, so as to include all terms of the order m^3 , however far the approximation be pushed, and without introducing any peculiar system of constants.

For this purpose it is necessary to suppose the disturbing function R

$$\begin{aligned}
 &= (R) + \left(\frac{dR}{d\xi}\right) \delta \xi + \left(\frac{dR}{da}\right) \delta a + \left(\frac{dR}{dc}\right) \delta c \\
 &+ \left(\frac{dR}{de}\right) \delta e + \left(\frac{dR}{d\omega}\right) \delta \omega + \left(\frac{dR}{d\gamma}\right) \delta \gamma + \left(\frac{dR}{d\alpha}\right) \delta \alpha \\
 &\xi = nt \qquad \frac{dR}{d\xi} = \frac{dR}{dc} \qquad n^2 a^3 = \mu;
 \end{aligned}$$

and to define $\delta \xi$, δa , δc , &c., to represent, not the total variation of the quantities ξ , a , c , e , &c., as in Poisson's paper, but that portion only which consists of arguments corresponding to those inequalities which are not depressed by integration, as will be presently explained. The effect of the secular inequalities of the constants c , ω and α , and also that of all the other inequalities of which the arguments are independent of c , are supposed to be already included in the quantities

$$(R), \left(\frac{dR}{da}\right), \left(\frac{dR}{dc}\right), \&c.$$

As the constant c in Poisson's notation always accompanies nt , all the arguments in the development of R may be properly represented by an expression of the form

$$i(nt + c) + jmt + lt + \beta,$$

where i and j are whole numbers or zero, l a certain multiple of m^2 depending upon the secular variation of the angles c , ω , α , and β a quantity rigorously constant, which accompanies lt , but which I shall in future omit to write down. Whenever $i = 0$ without j becoming $= 0$ at the same time, the corresponding inequality in the expressions for the elliptic constants is necessarily a multiple of m at least, and only when i and j are both equal to zero the corresponding inequality in each of those expressions may no longer be multiplied by m .

$$da = [a, c] \frac{dR}{dc} dt + [a, \omega] \frac{dR}{d\omega} dt + \&c.$$

$$d c = [c, a] \frac{d R}{d a} d t + [c, \omega] \frac{d R}{d \omega} d t + \&c.$$

The quantities between brackets may be considered as constant in the first approximation, and such that

$$[c, a] = -[a, c] \quad [\omega, c] = -[c, \omega].$$

Similar theorems exist with respect to all the constants, and it is upon this property that the theorem in question may be said to depend. If $a, e, \gamma, c, \omega, \alpha$, have the same signification as in Poisson's memoir (*Mémoire de l'Institut*, tom. xiii.)

$$\begin{aligned} [a, e] &= 0 & [a, \gamma] &= 0 & [e, \gamma] &= 0 \\ [e, \omega] &= 0 & [c, \gamma] &= 0 & [\omega, \alpha] &= 0; \end{aligned}$$

but these latter theorems do not appear to influence the proposition of which the proof is required.

In differentiating the disturbing function with regard to c , as indicated in the expression

$$d a = - \frac{2 d R}{a n d c} d t,$$

a being the semi-axis major, R must be differentiated only with respect to c , inasmuch as it was contained in R primitively, and not as it is introduced in further approximations by the variations of the elliptic elements. As, however, the secular variations and those multiplied by m and m^0 , which I have already included in

$$(R), \quad \left(\frac{d R}{d a} \right), \quad \left(\frac{d R}{d e} \right), \quad \&c.,$$

do not contain i or c , and as therefore c only occurs in the quantities

$$(R), \quad \left(\frac{d R}{d a} \right), \quad \left(\frac{d R}{d e} \right), \quad \&c.,$$

as it was introduced primitively*,

$$\begin{aligned} \frac{d R}{d c} &= \frac{d (R)}{d c} + \left(\frac{d^2 R}{d c^2} \right) \delta \zeta + \left(\frac{d^2 R}{d a d c} \right) \delta a + \left(\frac{d^2 R}{d c^3} \right) \delta c \\ &+ \left(\frac{d^2 R}{d e d c} \right) \delta e + \left(\frac{d^2 R}{d \omega d c} \right) \delta \omega + \left(\frac{d^2 R}{d \gamma d c} \right) \delta \gamma + \left(\frac{d^2 R}{d \alpha d c} \right) \delta \alpha \end{aligned}$$

$\delta \zeta, \delta a, \delta e, \&c.$, denoting here only that portion of the variations of $\zeta, a, e, \&c.$, which consists of periodical terms not independent of i , and therefore multiplied by the square of m at least.

* This remark applies solely to the constant c , and to no other.

$$\delta \zeta = -\frac{3n}{2a} \int \delta a \, dt,$$

and it is sufficient to take

$$\begin{aligned} \frac{da}{dt} = & \frac{1}{a^2 n} \left(3n \frac{d^2 R}{dc^2} \int \delta a \, dt - \frac{dR}{dc} \delta a \right) \\ & - 2 \left\{ \left(\frac{d^2 R}{da \, dc} \right) \frac{\delta a}{an} + \left(\frac{d^2 R}{dc^2} \right) \frac{\delta c}{an} + \frac{d^2 R}{de \, dc} \frac{\delta e}{an} \right. \\ & \left. + \left(\frac{d^2 R}{d\omega \, dc} \right) \frac{\delta \omega}{an} + \left(\frac{d^2 R}{d\gamma \, dc} \right) \frac{\delta \gamma}{an} + \left(\frac{d^2 R}{d\alpha \, dc} \right) \frac{\delta \alpha}{an} \right\}. \end{aligned}$$

This equation is similar in appearance only to the equation (A) of Poisson, p. 259, for the quantities δa , δc , δe , &c., are here differently defined.

I omit here all consideration of the terms

$$\frac{1}{a^2 n} \left(3n \frac{d^2 R}{dc^2} \int \delta a \, dt - \frac{dR}{dc} \delta a \right),$$

because the proof which Poisson has given in his *Mémoire sur le Mouvement de la Lune*, is completely satisfactory with reference to them.

In the terms $\frac{\delta c}{an}$, $\frac{\delta e}{an}$, &c., it is necessary to consider the variation of the quantities $[a, \omega]$, $[e, \omega]$, &c., and also that of the quantity $\left(\frac{1}{an}\right)$

$$d \cdot \frac{1}{an} = -\frac{da}{na^2} - \frac{dn}{na^2} = \frac{da}{2na^2} = -\frac{dR}{\mu \, dc} \, dt;$$

de contains the term

$$[e, \omega] \frac{dR}{d\omega} \, dt,$$

which may be taken as a type of others ;

$$\begin{aligned} \frac{1}{an} \int [e, \omega] \left(\frac{dR}{d\omega} \right) dt &= \int \frac{[e, \omega]}{an} \left(\frac{dR}{d\omega} \right) dt \\ &+ d \cdot \frac{1}{an} \int [e, \omega] \left(\frac{dR}{d\omega} \right) dt \\ &= \int \frac{[e, \omega]}{an} \left(\frac{dR}{d\omega} \right) dt - \frac{dR}{\mu \, dc} \int [e, \omega] \left(\frac{dR}{d\omega} \right) dt. \end{aligned}$$

It must be recollected that in this expression no terms are included in δe or $\int [e, \omega] \frac{dR}{dt}$ which are independent of c .

If $\left(\frac{dR}{de}\right)$, as defined and limited in the expression for R in p. 339, contains any term

$$A \cos (i (nt + c) + j m nt + l nt),$$

$d\omega$ contains the term

$$[a, \omega] A \cos (i (nt + c) + j m nt + l nt);$$

and considering now only the constant portion of $[a, \omega]$, $\delta\omega$ contains the term

$$[a, \omega] \frac{A}{(i + j m + l) n} \sin (i (nt + c) + j m nt + l nt).$$

Similarly, if $\left(\frac{dR}{d\omega}\right)$ contains the term

$$B \sin (i (nt + c) + j m nt + l' nt)$$

de contains the term

$$[\omega, e] B \sin (i (nt + c) + j m nt + l' nt),$$

δe contains the term

$$- [\omega, e] \frac{B}{(i + j m + l') n} \cos (i (nt + c) + j m nt + l' nt),$$

and these terms give, after well-known reductions, in $\int \frac{dR}{dc} dt$

$$[\omega, e] \frac{i A B}{n (i + j m + l) (i + j m + l')} \cos (l' - l) t,$$

which is evidently of the order m^4 , A and B being each of the order m^2 , and i of necessity not equal to zero.

I now proceed to consider the effect of the variation of the quantity

$$\frac{[e, \omega]}{a n},$$

which may be taken as the type of other similar quantities.

Let

$$\frac{[e, \omega]}{a n} = C + D \cos (i nt + j m nt + l nt)$$

$$\left(\frac{dR}{de}\right) = A \cos(i'nt + j'mnt + l'nt)$$

$$\begin{aligned} \frac{[e, \omega]}{an} \left(\frac{dR}{de}\right) &= CA \cos(i'nt + j'mnt + l'nt) \\ &+ \frac{DA}{2} \cos(i'nt - int + j'mnt - jmnt + l'nt - lnt) \\ &+ \frac{DA}{2} \cos(i'nt + int + j'mnt + jmnt + l'nt + lnt). \end{aligned}$$

$$\text{Let } \left(\frac{dR}{d\omega}\right) = B \sin(i''nt + j''mnt + l''nt)$$

$$\begin{aligned} \frac{[e, \omega]}{an} \left(\frac{dR}{d\omega}\right) &= CB \sin(i''nt + j''mnt + l''nt) \\ &+ \frac{DB}{2} \sin(i''nt - int + j''mnt - jmnt + l''nt - lnt) \\ &+ \frac{DB}{2} \sin(i''nt + int + j''mnt + jmnt + l''nt + lnt). \end{aligned}$$

$$\begin{aligned} \int \frac{[e, \omega]}{an} \left(\frac{dR}{d\omega}\right) dt \\ &= -CB \frac{\cos(i''nt + j''mnt + l''nt)}{(i'' + j''m + l'')n} \\ &- \frac{DB}{2} \frac{\cos(i''nt - int + j''mnt - jmnt + l''nt - lnt)}{(i'' - i + j''m - jm + l'' - l)n} \\ &- \frac{DB}{2} \frac{\cos(i''nt + int + j''mnt + jmnt + l''nt + lnt)}{(i'' + i + j''m + jm + l'' + l)n} \end{aligned}$$

$$\begin{aligned} - \int [e, \omega] \frac{dR}{de} dt \\ &= -CA \frac{\sin(i'nt + j'mnt + l'nt)}{(i' + j'm + l')n} \\ &- \frac{DA}{2} \frac{\sin(i'nt - int + j'mnt - jmnt + l'nt - lnt)}{(i' - i + j'm - jm + l' - l)n} \\ &- \frac{DA}{2} \frac{\sin(i'nt + int + j'mnt + jmnt + l'nt + lnt)}{(i' + i + j'm + jm + l' + l)n} \end{aligned}$$

$$\left(\frac{d^2 R}{de dc}\right) = -i' A \sin(i'nt + j'mnt + l'nt)$$

$$\left(\frac{d^2 R}{d\omega dc}\right) = i'' B \cos(i' n t + j'' m n t + l'' n t).$$

These terms give in $\frac{da}{dt}$

$$\left(\frac{d^2 R}{de dc}\right) \frac{\delta e}{a n} + \frac{d^2 R}{d\omega dc} \frac{\delta \omega}{a n},$$

and therefore

$$\left(\frac{d^2 R}{de dc}\right) \int \frac{[e, \omega]}{a n} \left(\frac{dR}{d\omega}\right) dt + \left(\frac{d^2 R}{d\omega dc}\right) \int \frac{[\omega, e]}{a n} \left(\frac{dR}{de}\right) dt$$

and hence the following terms :

$$\begin{aligned} & i' C B A \sin(i' n t + j' m n t + l' n t) \\ & \qquad \qquad \qquad \frac{\cos(i'' n t + j'' m n t + l'' n t)}{(i'' + j'' m + l'') n} \\ & + \frac{i' D B A}{2} \sin(i' n t + j' m n t + l' n t) \\ & \qquad \qquad \qquad \frac{\cos(i'' n t - i' n t + j'' m n t - j' m n t + l'' n t - l' n t)}{(i'' - i' + j'' m - j' m + l'' - l') n} \\ & + \frac{i' D B A}{2} \sin(i' n t + j' m n t + l' n t) \\ & \qquad \qquad \qquad \frac{\cos(i'' n t + i' n t + j'' m n t + j' m n t + l'' n t + l' n t)}{(i'' + i' + j'' m + j' m + l'' + l') n} \\ & - i'' C B A \cos(i'' n + j'' m n t + l'' n t) \\ & \qquad \qquad \qquad \frac{\sin(i' n t + j' m n t + l' n t)}{(i' + j' m + l') n} \\ & - \frac{i'' D B A}{2} \cos(i'' n t + j'' m n t + l'' n t) \\ & \qquad \qquad \qquad \frac{\sin(i' n t - i' n t + j' m n t - j' m n t + l' n t - l' n t)}{(i' - i' + j' m - j' m + l' - l') n} \\ & - \frac{i'' D B A}{2} \cos(i'' n t + j'' m n t + l'' n t) \\ & \qquad \qquad \qquad \frac{\sin(i' n t + i' n t + j' m n t + j' m n t + l' n t + l' n t)}{(i' + i' + j' m + j' m + l' + l') n}. \end{aligned}$$

The terms multiplied by $C B A$, which arise from the constant portion of $[e, \omega]$, have already been considered, the rest form four

pairs, of which the following is one, and may be taken as a type of the rest:

$$\frac{DBA}{4n} \left\{ \frac{i'}{i'' - i + j''m - jm + l'' - l} - \frac{i''}{i' + i + j'm + jm + l' + l} \right\}$$

$$\sin (i'n t + j'm n t + l' n t - i'' n t + i n t - j'' m n t + j m n t - l'' n t + l n t).$$

In order that this argument may be of the kind under consideration, we must have

$$\begin{aligned} i' - i'' + i &= 0 \\ j'm - j''m + jm &= 0, \end{aligned}$$

and the term becomes in that case

$$\frac{DBA}{4n} \left\{ \frac{i'}{i'' - i + j''m - jm + l'' - l} - \frac{i''}{i' + i + j'm + jm + l' + l} \right\} \sin (l' - l'' + l) n t$$

if i is not equal to zero, D is of the order m^2 , and the coefficient of $\sin (l' - l'' + l) n t$, after a fresh integration, remains of the order m^4 . If $i = 0$

$$i' = i''$$

the coefficient becomes

$$\begin{aligned} \frac{i' DBA}{4n} &\left\{ \frac{j'm + jm + l' + l - j''m + jm - l'' + l}{(i'' + j''m - jm + l'' - l)(i' + j'm + jm + l' + l)} \right\} \\ &= \frac{i' DBA \{jm + l' - l'' + 2l\}}{4n \{i'' + j''m - jm + l'' - l\} (i' + j'm + jm + l' - l)}; \end{aligned}$$

but unless $j = 0$, D is multiplied by m , and in either case, in consequence of the reductions which the numerator undergoes, the coefficient after a fresh integration remains of the order m^4 .

It remains now only to consider the terms of which the following is the type:

$$\left(\frac{d^2 R}{d e d c} \right) \frac{d R}{d c} \int [e, \omega] \left(\frac{d R}{d \omega} \right),$$

but as in δe are here only included terms of the order m^2 ,

$$\int [e, \omega] \left(\frac{d R}{d \omega} \right)$$

is of the order m^2 , or at least such portion of it as is supposed to be included in this expression; and as $\frac{d^2 R}{d e d c}$ and $\frac{d R}{d c}$ are each of the order m^2 , this term is of the order m^6 , which after a fresh integration remains of the order m^4 .

Hence we may conclude with safety, without having recourse to any peculiar system of constants, or to any precarious induction, that however far the approximation be carried, the variation of the semi-axis major in the Lunar Theory contains no term of long period multiplied by a power of m inferior to the fourth.

Sept. 21, 1840.

LI. *Some Observations on the aqueous Solution of Carbonate of Magnesia with excess of Carbonic Acid, and on the Salt which it affords by spontaneous Decomposition.* By JOHN DAVY, M.D., F.R.S.*

I HAVE been induced to institute some experiments on the solution of carbonate of magnesia in water strongly impregnated with carbonic acid gas, in consequence of the high repute, on very questionable grounds, which it has lately acquired as a medicine.

The solution I have used is that prepared and sold by Mr. Dinneford of New Bond-street, with the designation of Dinneford's Solution of Magnesia, and with the following recommendation on the label: "The great advantages of this elegant preparation are, that being in a *fluid* state and possessing all the properties of magnesia in general use, it is not likely to form dangerous concretions in the bowels; it corrects acidity and heart-burn effectually, without injuring the coats of the stomach, as carbonates of potash and soda are known to do; it prevents the food of infants turning sour, and in all cases it acts as a pleasing aperient, particularly adapted for females."

Such a recommendation I should not have thought it right to notice, were it an ordinary quack eulogy, and unsupported by certificates given by respectable medical men; and moreover were I not assured that great faith is placed by many persons in the asserted virtues of the preparation, and that the use of it is rapidly extending.

The first trials I subjected the medicine to, were made with a view to test the permanence of the solution; as by ex-

* Communicated by Sir David Brewster.

posure to the air in an open vessel, exposure to a temperature of 100° Fahrenheit, in a vessel loosely corked, and to the action of the air-pump under an exhausted receiver.

The result in each instance was very similar; carbonic acid gas escaped, or was expelled, and a salt was deposited in the form of minute prismatic crystals.

This separation of the magnesia in a solid form, on the disengagement of the excess of carbonic acid, was no more than might have been expected from the known nature of the compound, and the artificial manner in which it is formed by the condensation of the gaseous acid; and must be considered as quite incompatible with the declaration of its "fluid state" in the stomach and bowels, and sufficient ground to call in question the propriety of placing confidence in the preparation as a medicine, in preference to common carbonate of magnesia or calcined magnesia, than either of which it is so much more costly an article.

The prismatic salt deposited on the escape of the excess of carbonic acid, has been examined by several chemists; resting chiefly on the results of the experiments of Berzelius, and the late Dr. Henry, it has been considered as a hydrated carbonate of magnesia, composed of one proportion of magnesia, one of carbonic acid, and three of water.

From the experiments which I have made on it, it appears to be composed as follows; viz.

29·61 Magnesia.

32·22 Carbonic acid.

10·27 Water expelled at 212° Fahrenheit.

27·90 Water expelled by a higher temperature, as
by ignition.

100·00

or of one proportion and half of magnesia, and carbonic acid, one of water expelled at 212°, and three proportions of water expelled by a higher temperature. Compared with the common carbonate of magnesia, from the results which I have obtained operating on the latter, this appears to differ chiefly from the former in possessing half a proportion more of magnesia, and one proportion less of water, being composed of

41·52 Magnesia.

33·31 Carbonic acid.

17·47 Water expelled at 212°.

7·70 Water expelled at a higher temperature.

100·00

These results accord tolerably with those of other inquirers

who have examined this compound; the variation or want of perfect accordance, probably chiefly depends on the degree of dryness of the preparation examined, or on the quantity of water retained in the powder admitting of expulsion at 212° , which water being hygrometrical, at least in part, must vary with the degree of dryness of the atmosphere to which it is exposed.

The method by which these two compounds of magnesia and carbonic acid were analysed was a simple one, admitting of considerable accuracy.

The quantity of water expelled at a temperature of 212° was determined by exposure of an hour or more to the heat of a steam-bath; the quantity of carbonic acid, by acting on the compounds, very carefully weighed, by muriatic acid, saturated with carbonic acid, over mercury in a graduated tube; and the quantities of magnesia and of water expelled at a higher temperature than 212° by the action of a red heat, continued for two or three hours, till no further loss of weight was produced by a continuance of the high temperature. In estimating the proportion of carbonic acid, the calculation was made on the ground that 100 cubic inches of this gas weigh 47.262 grains.

A few words relative to the properties of the first-mentioned carbonate. Its tendency to crystallize is remarkable: however obtained, even when rapidly separated by the expulsion of the excess of carbonic acid by heat, it has been deposited in a crystalline form. This form is not obvious to the naked eye; but when the powder is examined by the microscope, each particle is found to be a distinct prismatic crystal. And the persistence of this form is no less remarkable; it is not destroyed by decomposition; the powder after ignition, after the expulsion of the whole of the water and carbonic acid, under the microscope shows no alteration; each particle is still prismatic, and when moistened with water is transparent.

It is asserted that this carbonate readily loses the water with which it is combined. In a dry atmosphere it loses a portion of the water, which perhaps may be considered as hygrometrical, and at the same time loses its transparency; but I find, as has been already remarked, that a temperature of 212° expels only one portion, and that a high temperature is requisite to expel the three remaining proportions, and which are probably the strictly chemically combined water.

It is also said that this compound is altered by the action of cold water, and by that of boiling water; that in one

instance a solution of bicarbonate of magnesia is formed, and an insoluble carbonate containing a smaller proportion of carbonic acid; and in the other, that the same insoluble subcarbonate is produced, but without the solution of bicarbonate, the proportion of carbonic acid required for this being expelled in the form of gas. The results of the trials I have made have not confirmed either of these conclusions. It has appeared to me to dissolve both in hot and in cold water, without undergoing any decomposition. I have not been able to obtain an insoluble subcarbonate of magnesia by acting on the prismatic salt by cold water, or carbonic acid gas from it by boiling water,—for instance, boiling it in distilled water in a retort connected with a mercurial pneumatic apparatus. It is true, that when this carbonate is thrown into hot water, there is a disengagement of air, but the air is common air mechanically entangled, not carbonic acid gas which had been chemically combined.

Both the hot solution and the cold, on evaporation, yielded the prismatic compound. 1000 grains of water at the temperature of 60° appear capable of holding in solution about four grains; thus 326.6 grains of the solution of carbonate, after the excess of carbonic acid gas had been expelled by the air-pump, afforded on spontaneous evaporation 1.5 grain of crystalline salt.

Whether this slight degree of solubility can be useful, considering the qualities of the compound as a medicine, or whether the crystalline spicular prismatic form which it assumes on separation of the excess of carbonic acid by which the carbonate was brought into solution can be injurious to the coats of the stomach, as a mechanical irritant, it is far from easy to determine; the probability is, reasoning analogically, that neither the one nor the other circumstance, medicinally considered, is of much consequence.

Fort Pitt, Chatham, Oct. 1, 1840.

LII. *An Abstract of Professor Daniell's Papers on the Electrolysis of Secondary Compounds, in the Philosophical Transactions for 1839 and 1840.*

IT has been long known that when a saline solution is subjected to the action of a galvanic current, both the water and the salt that it contains are resolved into their constituents; oxygen and the acid being evolved at the zincode, whilst hydrogen and the base appear at the platinode. The primary object of these researches was the determination of the relative proportions of these decompositions, and their relation

to the amount of electrolytic force in action, with a view to increase our knowledge of the constitution of saline bodies in general.

For this purpose an apparatus was constructed in the following manner, which Mr. Daniell calls "the double diaphragm cell."

"It consists of two cells formed of two glass cylinders, with collars at their lower ends, fitted by grinding to a stout glass tube bent into the form of the letter U, and firmly fixed on a wooden foot. The ends of this piece project a little into the interior of the two cylinders, the upper extremities of which are furnished with bent tubes for the collection of gases. A stout piece of platinum wire is ground to the upper part of each cell, to which an electrode of platinum or any other metal can be screwed on the inside, as occasion may require: the wires pass down upon the outside, and terminate in two mercury cups, by which connexion can be made, at pleasure, with the battery. Each cell will hold about seven cubic inches of liquid, and the connecting tube two inches. When the cell is charged, the connecting tube is filled with the liquid, and a piece of fine bladder tied over each end, so as perfectly to exclude the air. The bladders are firmly confined to their places by means of circular grooves ground round the ends of the glass tube. The cylinders are then carefully fitted to their places, and filled with the proper quantities of the solutions to be acted upon, and after the operation their contents are easily decanted." The quantity of liquid in each cell during the experiment was about 4.5 cubic inches. The power employed was that of a small constant battery of Mr. Daniell's construction, containing thirty cells six inches in height, with tubes of porous earthenware, charged in the ordinary manner. (See *Phil. Trans.* for the year 1836.)

From this battery the current was made to pass through the apparatus just described, filled with a solution of the salt to be examined, say sulphate of soda. A common voltameter charged with dilute sulphuric acid, was also included in the circuit, so that the mixed gases evolved might be collected, in order to ascertain the exact amount of electrolytic force really in circulation.

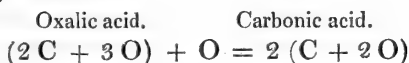
The gas given off from each side from the double diaphragm cell was also collected, and the united bulk of the oxygen and hydrogen so evolved was found to be exactly equal to the volume of the mixed gases collected from the common voltameter. From numerous experiments it was found, that on decanting the saline liquid from each cell, and care-

fully neutralizing with acid or alkali, as the case might require, the quantity of salt decomposed was almost, if not exactly, equivalent to the gas evolved.

(Thus if 11·8 cubic inches of oxygen had been evolved from the zincode, and 23·6 cubic inches of hydrogen from the platinode [the results of the decomposition of 4·5 grains of water], by neutralization it was found that the zincode cell contained about 20 grains of free sulphuric acid, and the platinode 16 grains of free soda, numbers which are equivalent to the quantity of water decomposed; meantime from the single voltameter 35·4 cubic inches of mixed gases [also the result of the decomposition of 4·5 grains of water] had been collected).

These experiments were repeated upon *sulphate* of potass, *phosphate* of soda, *sulphate* of *ammonia*, showing the analogy of ammonia with metallic salts, and *nitrate* of potass, with corresponding results. The determination of the quantity of alkali in the latter instance was not possible, owing to the formation of a quantity of ammonia at the platinode, from the reaction of the disengaged hydrogen upon the nitric acid of the salt.

The *carbonates* were examined in a similar manner with like results. *Oxalate* of ammonia yielded nothing but pure carbonic acid at the zincode, whilst hydrogen and ammonia appeared at the platinode. The reason of this is evident from the following formula:—



the equivalent of oxygen afforded by the electrolytic decomposition of the salt being just sufficient to convert one equivalent of oxalic acid into two equivalents of carbonic acid.

Sulphovinate of potassa was decomposed also in equivalent proportions, the acid and oxygen passing to the zincode whence the gas escaped uncombined—hydrogen and potassa being as usual developed at the platinode. From the preceding experiments it appears, “that in the electrolysis of a solution of a neutral salt in water a current which is just sufficient to separate single equivalents of oxygen and hydrogen from a mixture of sulphuric acid and water, will separate single equivalents of oxygen and hydrogen from the saline solution, while single equivalents of acid and alkali will make their appearance at the same time at the respective electrodes.”

These relations were found equally to hold good, whether the oxygen was allowed to escape from the zincode of the double diaphragm cell, or whether it was absorbed by an

electrode of copper, or of zinc, as in the ordinary cells of the battery.

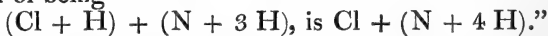
Further experiments showed that whenever dilute sulphuric acid is used, there is a transfer of acid towards the zincode, and the determination of the proportions in which such a transfer occurs led to some curious results, to which we must presently revert.

In order, however, to remove the ambiguity which might thus possibly be conceived to arise from the employment of dilute sulphuric acid as the measure of the electrolytic force, the following arrangement was substituted for the ordinary voltameter: a green glass tube (into the bottom of which, as platinode, was welded a weighed platinum wire) was filled with chloride of lead, maintained in a state of fusion by a spirit-lamp; the corresponding zincode was formed of plumbago. At the termination of the experiment the tube was broken, the wire and adhering button of lead weighed; and the result showed that "the same current which is just sufficient to resolve an equivalent of chloride of lead, which is a simple electrolyte unaffected by any associated composition, into its equivalent ions, produces the apparent phenomena of the resolution of water into its elements; and at the same time of an equivalent of sulphate of soda into its proximate principles."

Aqueous solutions of the chlorides were next tried, as the simple constitution of this class of salts promised to throw light upon the nature of the electrolysis of secondary compounds.

A weighed plate of pure tin was made the zincode of the double cell, which was charged with a strong solution of chloride of sodium, and a tube of fused chloride of lead, as before, included in the circuit; not a bubble of gas appeared on the tin electrode, and no smell of chlorine was perceptible, but hydrogen in equivalent proportion to the quantity of tin dissolved was given off at the platinode, and the cell contained an equivalent proportion of free soda. One equivalent of lead was reduced in the voltameter tube.

Muriate of ammonia treated in the same way gave precisely similar results, proving it to be "an electrolyte, whose simple anion was chlorine, and compound cathion nitrogen with 4 equivalents of hydrogen. Its electrolytic symbol, therefore, instead of being



Strikingly confirming the hypothesis of Berzelius of the base $(\text{N} + 4 \text{H})$ called ammonium.

In discussing the results of all these experiments, we must bear in mind the fundamental principle, "that the force which we have measured by its definite action at any one point of a

circuit cannot perform more than an equivalent proportion of work at any other point of the same circuit.”—“The sum of the forces which held together any number of ions in a compound electrolyte, could, moreover, only have been equal to the force which held together the elements of a single electrolyte, electrolyzed at the same moment in one circuit.”

In the electrolysis of the solution of sulphate of soda, and many of the other salts, “water seemed to be electrolyzed; at the same time acid and alkali appeared in equivalent proportion with the oxygen and hydrogen at the respective electrodes.”—“We must conclude,” from the above-mentioned principle, “that the only electrolyte which yielded was the sulphate of soda, the ions of which, however, were not the acid and alkali of the salt, but an anion composed of an equivalent of sulphur and four equivalents of oxygen and the metallic cation sodium; from the former, sulphuric acid was formed at the anode by the secondary action and evolution of one equivalent of oxygen; and from the latter, soda at the cathode by the secondary action of the metal and the evolution of an equivalent of hydrogen.”

To avoid circumlocution (but only when speaking of electrolytic decomposition), Mr. Daniell proposes to adopt the word *ion*, introduced by Dr. Faraday, as a general termination to denote the compounds which in the electrolysis of a salt pass to the zincode, and that they should be specifically distinguished by prefixing the name of the acid slightly modified, as is shown in the following table:—

Ordinary chemical formula.	Electrolytic formula.	
Sulphate of copper $(S+3O)+(Cu+O) = (S+4O)+Cu$.	Oxysulphion of copper.	
Sulphate of soda $(S+3O)+(Na+O) = (S+4O)+Na$.	Oxysulph. of sodium.	
Nitrate of potassa $(N+5O)+(Ka+O) = (N+6O)+Ka$.	Oxynitron of potassa.	
Phosphate of soda $(P+3\frac{1}{2}O)+(Na+O) = (P+3\frac{1}{2}O)+Na$.	Oxyphosph. of soda.	

The following experiments seem to remove all doubt that the view just sketched is correct; they were, in fact, suggested to Prof. Daniell by the theory itself.

“A small glass bell, with an aperture at top, had its mouth closed by tying a piece of thin membrane over it. It was half filled with a dilute solution of caustic potassa, and suspended in a glass vessel containing a strong neutral solution of sulphate of copper, below the surface of which it just dipped. A platinum electrode, connected with the last zinc rod of a large constant battery of twenty cells, was placed in the solution of potassa; and another, connected with the copper of the first cell, was placed in the sulphate of copper immediately under

the diaphragm which separated the two solutions. The circuit conducted very readily, and the action was very energetic. Hydrogen was given off at the platinode in a solution of potassa, and oxygen at the zincode in the sulphate of copper. A small quantity of gas was also seen to rise from the surface of the diaphragm. In about ten minutes the lower surface of the membrane was found beautifully coated with metallic copper, interspersed with oxide of copper of a black colour, and hydrated oxide of copper of a light blue.

“The explanation of these phenomena is obvious. In the experimental cell we have two electrolytes separated by a membrane, through both of which the current must pass to complete its circuit. The sulphate of copper is resolved into its compound anion, sulphuric acid + oxygen (oxysulphion), and its simple cation copper: the oxygen of the former escapes at the zincode, but the copper on its passage to the platinode is stopped at the surface of the second electrolyte, which for the present we may regard as water improved in its conducting power by potassa. The metal here finds nothing by combining with which it can complete its course, but being forced to stop, yields up its charge to the hydrogen of the second electrolyte, which passes on to the platinode, and is evolved.

“The corresponding oxygen stops also at the diaphragm, giving up its charge to the anion of the sulphate of copper. The copper and oxygen thus meeting at the intermediate point, partly enter into combination, and form the black oxide; but from the rapidity of the action, there is not time for the whole to combine, and a portion of the copper remains in the metallic state, and a portion of the gaseous oxygen escapes. The precipitation of blue hydrated oxide doubtless arose from the mixing of a small portion of the two solutions.”

Nitrate of silver, nitrate of lead, proto-sulphate of iron, sulphate of palladium, and proto-nitrate of mercury, were similarly treated, and afforded analogous results, somewhat modified by the nature of the metallic base. Sulphate of magnesia was subjected to the same process, in hopes of finding magnesium, but magnesia alone was deposited.

The theory of ammonium, as proposed by Berzelius, and the hypothesis of Davy developing the general analogy of all salts, whether derived from oxyacids or hydracids, may by this evidence, especially when taken in conjunction with the recent researches on the constitution of organic bodies, be considered as almost experimentally demonstrated*.

The bisalts yield results which at first sight do not accord

* See Additional Note at the end of the present Number among the Miscellaneous Notices.

with the preceding deductions; a strong solution, for example, of pure crystallized bisulphate of potassa was made, and its neutralizing power carefully ascertained by the alkalimeter. Evaporation and ignition with carbonate of ammonia gave the quantity of neutral sulphate yielded by a certain measure of the solution. An equal measure was then placed in each arm of the double diaphragm cell, and the current passed through till 70·8 cubic inches of mixed gas were collected; half the solutions from the zincode and platinode were then separately neutralized, and half evaporated and ignited in the vapour of carbonate of ammonia.

It was then found that the zincode had gained 18 grains; the platinode lost 19 of free acid: of potassa the zincode had lost 9·9 grains, and the platinode gained an equal quantity: thus, though the solution conducted very well, not more than one-fifth of an equivalent of the potassa was transferred to the platinode, as compared with the hydrogen evolved, while half an equivalent of acid was transferred to the zincode when where a whole equivalent of oxygen was evolved. Mr. Daniell remarks upon this experiment,—

“ I think we cannot hesitate to admit that, in this case, the current divided itself between two electrolytes, and that a part was conducted by the neutral sulphate of potassa, and a larger part by the sulphuric acid and water. It is a well-known fact that the voltaic current will divide itself between two or more metallic conductors in inverse proportion to the resistance which each may offer to its course; and that it does not in such cases choose *alone* the path of least resistance. I am not aware that such a division of a current between two electrolytes in the same solution has ever before been pointed out, but analogy would lead me to expect it.” These considerations enable us to explain some apparent anomalies in the electrolysis of diluted sulphuric acid and alkaline solutions.

When diluted sulphuric acid was placed in the double diaphragm cell, and the current transmitted, some of the acid passed to the zincode; but from numberless experiments it appeared that this quantity scarcely ever exceeded the proportion of one-fourth of an equivalent as compared with the hydrogen evolved. Mr. Daniell thought possibly this might be owing to the acid being mechanically carried back to the platinode, as in all cases there is a mechanical convection of the liquid from the zincode to the platinode, and this is the greater in proportion to the inferiority of its conducting power. If, however, this deficiency of acid were owing to a mechanical re-transfer, mechanical means, such as increasing the number of diaphragms, would stop it; the proportion,

however, was even under these circumstances still maintained. No difference was observed whether the oxygen was allowed to escape as from a zincode of platinum, or was absorbed by copper or zinc; the metals, of course, being dissolved in proportions equivalent to the hydrogen developed at the platinode. Solution of potassa, baryta, or strontia, similarly treated, exhibited a transfer of about one-fourth of an equivalent towards the *platinode*.

These curious results are easily explained by supposing that the solution is a mixture of two electrolytes; with sulphuric acid they are $H + (S + 4 O)$, oxysulphion of hydrogen ($H + O$) water; the current so divides itself that three equivalents of water are decomposed, and one equivalent of oxysulphion of hydrogen. Analogous changes occur with the alkaline solutions, the alkaline metal passing as usual to the platinode.

LIII. *On Magneto-electric Induction; in a Letter to M. Gay-Lussac.* By MICHAEL FARADAY, D.C.L., F.R.S.

[Continued from p. 289, and concluded.]

I REPRESENTED this state of things under a general form, in the figures *ij* annexed to the memoir, which, as to the arrows, the designation of the parts, &c. &c., I have made to correspond, as well as I could, with fig. 2. plate iii. of the Italian philosophers' memoir (plate ii.). I proceed to show how it agrees with the galvanometrical results obtained by them, and how far with their *conclusions*.

With regard to the galvanometrical results, my figure might be used instead of theirs, without occasioning any difference, and I have no reason to say that they are inexact. Relatively to "one of those consequences," which arises from "the immediate inspection of the arrows which mark the currents in the two regions of the disc," or from any other attentive and experimental examination, we see that the currents *n, n, n*, on entering, instead of being in a contrary direction to those which are in the parts *s s s*, which recede, follow exactly the same direction; that is, that as to the general movement near the pole they go from above below, or from the circumference towards the centre, transversely to the lines that the different parts describe in their course; and at a great distance (F. 92.) on each side of the pole they are in the contrary direction. In proportion to the nearness to the pole of a part of the line described by a point, it is traversed by a current, which commences, and increases in intensity

until it reaches the shortest distance, or a little beyond, on account of time entering as an element into this effect. Afterwards, by reason of the increasing distance, the current diminishes in intensity without ever altering its direction relatively to its proper course. It is only when it arrives at the parts most distant, at which the electricity excited is discharged, that a current is manifested in an *opposite* direction, or in one more or less oblique. I apprehend that it is wholly useless to speak of the partial alterations in the direction of the currents through the parts that are the nearest to the centre, or to the circumference; two or three curves that I have rudely traced will show in what directions these alterations take place.

The second consequence arising from the memoir of the Italian philosophers is, that "the direction of the currents upon the parts that enter is contrary to that of the producing currents; (that is, of those that are considered as existing in the magnet) while on the other side the direction in the two systems is identical." This assertion is exactly contrary to the reality (F. 117.). In figures 1. and 2. I have indicated, by means of arrows, the direction of the currents in the magnetic pole, which is the same as the direction given by Messrs. Nobili and Antenori in fig. 1. pl. iii. But my figure 2, as well as the indications of the galvanometer, shows evidently that the currents in the parts that enter *n, n, n*, when they approach the magnet, pass through in the same direction as the current in this side of the pole of the magnet; and that the currents in the parts that recede *s, s, s*, follow a direction contrary to those supposed to exist in the side of the magnetic pole from which they recede.

I may be mistaken, but it appears to me that Messrs. Nobili and Antenori suppose that circular currents are excited in the part of the metal adjacent to the pole, in absolutely the same manner as those formed in the helix, when it is made to approach the magnet, and that when this part of the disc recedes, the circular currents are somehow reversed, as occurs in the helix during its recession from the magnet. A passage in their first paper, and another at the end of page 284, appear to imply that such is their opinion. This idea occurred to me above a year ago, but I soon saw from numerous experiments, some of which I have just referred to, that it was by no means satisfactory; and when I had fully verified that the action of the helix in its approach to, and recession from the pole was wholly explained (F. 42.) by the law assigned (F. 114.), I was forced to abandon my previous ideas.

The memoir afterwards proceeds (p. 288.) to explain the

phænomena of Arago's revolving disc ; but as I have shown that the theory is in general based upon two conclusions contrary to truth, it is unnecessary to make a minute examination of it. It is impossible for it to exhibit the phænomena with exactitude. Those who are anxious for full satisfaction on the subject, may decide, by means of a few experiments, whether the opinions which I put forth in the paper which first announced the discovery of these currents be true, or whether the Italian philosophers were justified in declaring that I was in error, and that they had published more just ideas on the subject.

Everybody knows that when M. Arago published his remarkable discovery, he said the action of the disc upon the magnet was resolvable into three forces: the **FIRST**, perpendicular to the disc, which he found to be repulsive: the **SECOND**, horizontal and perpendicular to the vertical plane containing the radius beneath the magnetic pole; this is a tangential force, and occasions the rotation of the pole with the metal: the **THIRD**, horizontal and parallel to the same radius; it becomes null at a certain point towards the circumference; but when nearer the centre, it has a tendency to impel the pole towards the centre; and when nearer the circumference, to impel it in the contrary direction.

At page 289, Messrs. Nobili and Antenori give an explanation of the first of these forces. As has been already said, these gentlemen consider that the parts adjacent to the magnet have currents contrary to those which are found near the pole to which they approach, and consequently they are repulsive; and they consider that the parts that recede have currents identical in direction with those which are near the magnet from which they recede, and consequently these parts are attractive. The sum of each of these various forces is equal one to the other, but in what relates to the needle or magnet this distribution differs; for "the repulsive forces being the nearest, invade the disc as far as the parts under the needle, and thus obtain a preponderance over the action of the contrary forces, which are exerted more obliquely, and at a greater distance. In short, it is only a part of the repulsive forces which is balanced by the attractive forces; the remainder meets with no opposition, and it is this remainder that produces the effect."

But I have shown in this letter, that the currents in the parts adjacent or distant are exactly contrary to what is supposed by Messrs. Nobili and Antenori; and that consequently where they expect attraction they would find repulsion, and attraction where they expected repulsion; so that,

following their opinion, corrected by experiment, the result should be *attraction* instead of *repulsion*. But Arago was right in saying that it is repulsion; and consequently the theory of the effect given cannot be the true one.

My views upon the subject in question may be found in my first paper. I examined whether it were possible or probable (F. 125.) that time could be a necessary element for the development of the maximum current in the metal. In this case the resultant of all the forces would be in advance of the magnet, when the plate was rotated, or in the rear of it, if it (the magnet) were rotated; and a line joining this resultant with the pole would be oblique to the plane of motion; then the force in the direction of this line might be resolved into two others, one parallel, the other perpendicular to the plane of movement or rotation; the latter would be a repulsive force, producing an effect analogous to that remarked by M. Arago.

The second force is that which occasions the magnet and the disc mutually to follow each other. Referring to page 290, fig. 1. or 2. (my figure 2. may also be made use of,) we read, "Forces of attraction exist in s, s, s , towards which it (the magnet) is attracted, and repulsive forces in n, n, n , which impel it in the same direction;" consequently the magnet moves either after, or with the metal; but the currents, and consequently the forces, are exactly contrary to what has been supposed, as I have just shown; the magnet and the disc should therefore move in opposite directions, if the forces act in the manner that has been supposed. But as they do not move, in fact, in opposite directions, it is evident that the theory which explains their movement by reversing the facts must be itself erroneous.

The third force is that which has a tendency to remove the magnetic pole either towards the centre or the circumference, on each side of a neutral point situated upon the radius above which the magnet is placed; this effect is described at page 281, and in fig. 4, which accompanies the memoir, and which I believe to be perfectly correct. The memoir proceeds to explain the effect by referring to the repulsive force admitted (p. 289.) to account for the first effect observed by Arago, viz. the vertical repulsion of the disc; and supposing that this repulsive force be distributed over a certain extent of the disc, beneath the magnet, it is established (p. 292. fig. 5.) that if the pole be situated very near the circumference, the portion of the body whence this force emanates will be lessened, being cut by the circumference itself; consequently the parts that are nearest to the centre are more powerful, and impel the pole in an outward direction; while if the pole be placed very near

the centre, the extent whence the force emanates will pass it; and as this part in excess is considered, though erroneously, as inactive, the portion situated towards the circumference is more powerful, and impels the pole towards the centre.

Two or three slight objections present themselves to this opinion, but they are nothing, so to speak, in comparison with that which arises, when it is recollected, that in conformity with the author's own ideas upon the action of currents, the error with respect to the direction of those which are excited near the pole obliges us to substitute *attraction* for *repulsion*, as I have already shown when treating of the first of these forces: consequently all the movements which are connected with the third force would be in a direction contrary to those that are actually presented; and the theory which, when corrected by experiments made with the galvanometer, indicates such movements, must be abandoned.

Page 292 of the memoir refers to Mr. Faraday's "second law." As I have already said, I never stated those three assertions as laws. I really regret extremely that a letter that was never intended to convey minute details, but merely a few facts, selected in haste from a multitude described previously in the memoir read before the Royal Society,—I regret that this letter, which I never expected to see in print, should have led the Italian philosophers into error. However, after having examined anew all the facts, I do not see that I am in any degree responsible for the error they have committed, as having advanced fallacious results; nor, as far as the memoir is concerned, for not having given to the scientific world the most complete details at the earliest period possible.

I have not yet published my views as to the cause of the third force described by M. Arago; but as Messrs. Nobili and Attenori, when giving the hypotheses, which I justly regard as inexact, say (p. 293.), "In fact, what other hypothesis can reconcile the verticality that the needle preserves in the two positions n , s , n'' , s'' , (fig. 4) with the fact of the repulsion from below, above which raises the needle in the second position s'' , n'' ?"—I am induced to offer another hypothesis, premising, however, that the directions and forms that I shall trace, as those of the excited magneto-electric currents, are to be considered only as general approximations.

If a piece of metal, large enough to contain without distortion all the currents which may be excited in its whole extent by a magnetic pole placed above it, be moved in a rectilinear direction beneath the pole, then an electric current will move across the direction of its motion, in the parts immediately adjacent to the pole, and will return in the opposite direction

on each side in the parts which, being more distant from the pole, are subject to a feebler inductive force: the current will thus be completed or discharged (see fig. 3.). Let A B C D represent a piece of copper moving in the direction of the arrow E, and N the north end of the magnet placed above; currents of electricity will be produced in the piece of metal; and though they undoubtedly extend from the part below the pole to a great distance around (F. 92.), and at the same time diminish in intensity and alter in direction as they recede thence, yet the two circles may serve to represent the resultant of these currents; and it will be evident that the point of most intense action will be where they touch, and immediately under the magnetic pole, or, on account of the time required, a little in advance of it. Hence that portion of the forces which acts parallel to the plane of the metal will carry the pole forward in the direction of the arrow E, because the forces are equally powerful on the side of the pole A B, as on the side C D; and this portion, which on account of the time necessary for the production of the currents excited is perpendicular to the direction of the metal, will be, as we have said, repulsive, and have a tendency to impel the pole upwards and away.

But suppose that instead of the metal moving in a rectilinear direction, a circular disc revolving upon its axis be substituted; and then let us consider, in the first place, the case of the magnetic pole placed upon its centre (fig. 4.); there is then no production of electric currents, not because there is no tendency to their formation, for I have stated in this letter, and shown in my memoirs (F. 149. 156. 217.), that from the time the disc begins to move, currents are also ready to move; but because they have a tendency to be formed in the direction of radii from the circumference to the centre; and as all the parts are equally influenced, none of them having an excess of power over the others, and all equally distant from the centre, no discharge can take place, and consequently no current can be developed. As no current can exist, no effect dependent on the action of a current upon the pole can be produced, and consequently there is then neither *revolution* nor repulsion of the magnet. Hence the cause of the *verticality without repulsion* which occurs at this place.

Let us now consider the case in which the pole of the magnet, instead of being placed over the centre of the metal, is at one of its sides, as in N, figure 5. The tendency to form electric currents is due to the movement of the parts of the disc *through* the magnetic curves (F. 116. 217.), and when these curves are of equal intensity, the electric currents increase in

force in proportion to the increase of rapidity in the motion of the parts of the disc that intersect the magnetic curves (F. 258.). Let us now trace a circle $a b$ around a magnetic pole as a centre, and it will represent the projection of magnetic curves of *equal intensity* upon the disc; a and b are points situated at an equal distance from the pole, in the passing radius which is immediately under the pole; but as the part or point a passes by the pole with much greater velocity than the part b , the intensity of the electric current which is excited in this part a is proportionably greater. This is also true for the points in any other radius of the revolving plate cutting the circle $a b$, and true likewise for any other circle traced round N as a centre, and representing consequently magnetic curves of equal intensity; with the exception, that when the circle extends beyond the centre C of the revolving disc, as to $c d$, instead of the existence of a feebler current at the point d than at the point c , there is then a tendency to produce an opposite current.

The natural consequence of these actions of the different parts is, that as the sum of the forces tending to produce the electric current in the direction from c to d is greater on the side c of the magnetic pole than on the side d , the curvature or return of these currents by the right and left also commences on this side; and then the two circles, which as before may be considered as representing the resultants of these currents, do not come into contact exactly under the pole, but at a greater or less distance from it, towards the circumference, as in figure 6.

This circumstance of itself would not occasion any movement in a pole restrained in its motion to the direction of the radius only; but being combined with that which results from the *time* necessary for the development of the current, and to which reference has been already made, as explaining the *first* of the three forces by which M. Arago exhibits the action of the magnetic pole and disc, it will, I hope, fully elucidate all the effects that we are investigating, and will also prove the influence of time as an element. Let c (fig. 7.) be the centre of a revolving disc, and rc a part of the radius under the magnetic pole p ; the contact of the two circles representing the currents is, as we have just seen, on the side of the pole beyond the centre c ; but on account of the element of time and the direction of the rotation R of the plate of metal, it is also a little to the left of the radius rc ; so that the pole is brought under the action of the two orders of currents, not symmetrically but obliquely. The necessary consequence is, that if it be free to move in the direction of the radius, and in that di-

rection alone, it will move towards the centre c , for the currents produced by a marked pole (north) are exactly such as by their action on the pole to impel it in that direction.

This relation of the currents to the pole by which they are generated, may be proved by experiment as easily as by calculation. I have shown (F. 100.) that when a pole marked north is above a disc revolving in the direction of the arrows R , in the figures annexed either to Messrs. Nobili and Antenori's memoir or to mine, the currents (indicated by the circles) are as is represented in figures 3, 6, or 7. Upon arranging a metal wire which would conduct the currents in this double direction (fig. 8.), and placing over it a marked pole (north) capable of moving only in a parallel direction to rc , at any point in the line rc , I found it had not any tendency to move. There was also another line perpendicular to the first, and which crosses it at the point of contact of the circles, in which the pole had no tendency to move. If placed in any other situation than upon these two lines, it moved either in one direction or the other; and when placed in the positions marked 1, 2, 3, 4, it moved in the direction of the arrows represented at those points. Now the position of the pole, with regard to the currents in Arago's experiment, when the magnet and the disc are arranged as in figures 5 and 7, is exactly that of the point 1 in fig. 8, and hence that pole has a tendency towards the centre C .

We will now direct our attention to the result obtained if we gradually move the pole from the centre towards the circumference. Let figure 9. represent this new condition at a given time, as figure 5. represented the first state; it is evident that the velocity of the parts $a b$ of the radius beneath the pole, will not differ from each other so much as they did previously, being only about 3 : 2 instead of 6 : 1; and the difference will also be less with all the curves of equal intensity comprised in this circle. This occasions the situation of the pole, and the place of contact of the circles representing the currents (fig. 7.) mutually to approach in the direction of the line rc , and necessarily carries the pole (fig. 8) nearer to the neutral line li . If we examine the second circle cd , fig. 9, of magnetic curves of equal intensity, it will be seen, that as the disc does not extend to c , or even beyond a , there is nothing to add to the force of the current upon that side of the pole, while at d the radius, by moving through the magnetic curves, adds to the intensity of the current excited at b , and everywhere else on that side of the pole, and may easily, according to the position of the pole upon the metal plate (that is, according as it is nearer or further from the edge),

render their sum equal or greater than the sum of the forces on the other side, or that towards the circumference. If the sum of the forces be equal, then the pole will be somewhere in the line li , as at 5, fig. 8, and will have no tendency either towards the centre or the circumference, though its tendency to move with the disc or above it remains the same. Or if the sum of the forces be greater on the side d , fig. 9, than on the side c , then the pole will be in the position 2, fig. 8, and will be impelled outwards in the direction of the radius, in conformity with Arago's results.

Besides this cause of alteration in the motion of the pole parallel to the radius, and which is dependent on the position of the pole near the circumference, there is another cause that occurs, I apprehend, at the same time, and assists the action of the first. When the pole is placed towards the edge of the disc, the discharge of the currents excited behind is thrown against the side of the edge, from the absence of conducting matter; thus, in fig. 10, instead of having the regular form of the figures 7 and 8, the currents are deflected in their course towards the circumference, while they have all necessary latitude for their movement in the parts towards the centre; this of itself would cause the point of greatest force to fall a little nearer the centre than the projection of the axis of the magnetic pole, and assist in placing the pole in the position 2, fig. 8. I have such confidence in this opinion, that though I have not had opportunity to make the experiment myself, yet I venture to predict, that if instead of employing a revolving disc, a lamina or plate of metal, five or six inches broad, as A, B, C, D, fig. 9, were caused to move in a rectilinear direction conformably to the arrow, under a magnetic pole situated at a , the pole would have a tendency to move forward with the metal as well as above, but neither towards the right nor left; while if the pole were placed above the point b , it would be directed towards the edge A B; or if it were placed above c , it would have a tendency to move towards the edge C D.

Having thus replied to the question, "What other hypothesis"?, &c. proposed by the authors of the memoir at p. 293, I shall continue my examination of the memoir itself. At p. 294 the error relative to the nature of the currents, that is their supposed inversion, is repeated. The effect described is sure enough with a helix, and some particular forms of apparatus; but the simple and elementary current generated by the passage of a wire in front of a magnet is not reversed when the metal wire recedes. (F. 171. 111. 92.)

At p. 295 is the supposition that when the rotation is slow

“the revolution of the currents is circumscribed within narrow limits, and there is *little to add* to the results that form the basis of the [our] whole theory;” but that when the motion is rapid the currents envelope the whole disc, “so as to become a species of labyrinth.” For my part I believe the currents have the same general direction as has been assigned to them in the figures, whether the rotation be slow or rapid; the only difference is an increase of velocity.

A circumstance is then selected which is really simple, though it may at first appear complicated; namely, that in which the opposite poles are adjusted over a disc in one diameter, but towards the opposite edges on each side of the centre. This circumstance, with the direction of the movement and the current produced, is exhibited in fig. 7 of Messrs. Nobili and Antenori’s memoir. It is unnecessary to quote pages 296 and 297, which contain the explanation of this figure, but I shall refer to fig. 12, which corresponds to it, and is in conformity with my views and experiments, so that the two may be compared together. It is very satisfactory to me to find, that in this part of the memoir, as well as in the first, there is no important result of experiment contrary to my published opinions, though I am very far from adopting the conclusions that have been drawn from them.

If figure 12 be examined, it will be instantly seen that it results in the most simple manner from the action of the two poles. Thus, as far as the upper or north pole only is concerned, the currents are as in figure 6. But as with the north pole, the current determined by it moves from the circumference towards the centre, so with the south pole, in the same or corresponding position, the currents move from the centre to the circumference (F. 100.); and consequently in fig. 12 they are continued along the diameter N, S, through the centre of the plate, to return in the direction of the arrows upon the sides E, O. The points upon which I find my views to disagree with the indications of the galvanometer obtained by Messrs Nobili and Antenori are, first, the direction of the currents at N and S, which is contrary to what I obtained; and, secondly, the existence of any oblique axis of power, as P, Q, in their figure 7.

The memoir concludes, at least as far as I am concerned, at page 298, by again mentioning the error (but not as an error) relative to the revolving disc, which becomes a *new electrical machine*. At the commencement, the authors being little conversant with the principles under the influence of which such a result is obtained, deny it; and though they say here, “What shall we say after the *new observations* that

we have made during the continuation of our researches?"—I am not disposed to alter anything that I have published; I have even more confidence than before in my own views; for had their observations been in agreement with the results which I had obtained, I should have had great reason, after my examination of their papers, to fear that my own ideas were erroneous.

I cannot conclude this letter without again expressing my regret at having been obliged to write it; but if it be recollected that Messrs. Nobili and Antenori's memoirs were written and published *after my* original memoirs; that their last paper appeared even in the *Annales de Chimie et de Physique* after mine; and that it had consequently the appearance of advancing the science further than I had done; that both papers accuse me of error in experiment and theory, and also of dishonesty; that the last of these papers is dated in March, and though it is now December, has been followed by no correction or retractation on the part of the authors; and that I sent them several months ago (at the same time that I forwarded them to you and others,) copies of my original memoirs, and of my notes to a translation of their first memoir; and if it be considered that, after all, I have not to reproach myself with the errors of which I am accused, and that these gentlemen's memoirs are so framed as to compel me to reply to their objections;—I hope that no one will say that I have written too hastily what might have been avoided; or that I should have shown respect for the truth, and done justice to my own publications, or to this branch of science, if, being aware of such important errors, I had not called attention to them. I am, my dear Sir, yours very sincerely,

M. FARADAY.

LIV. *On the Law of Storms.* By H. W. DOVE.

To Richard Taylor, Esq.

Editor of the Philosophical Magazine and Journal.

DEAR SIR,

IN the year 1828, I published in Poggendorff's *Annalen*, vol. xiii. p. 596, a memoir "On Barometric Minima," in which I established the fact, that the storm which accompanies a great depression of the barometric column is a vast whirlwind, which in the northern hemisphere proceeds from S.W. to N.E. The example there more especially investigated is the storm of the 24th of December, 1821, the centre of

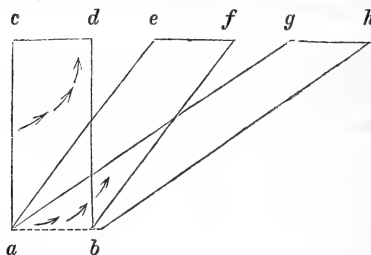
which travelled from Brest to Cap Lindenaes in Norway. The rotation in this whirlwind was in the direction S.E. N.W., consequently on the S.E. side of the storm, that is to say, in France, Holland, Germany, Italy, Denmark, and Prussia the weather-vane veered from S.E. to S.W. and W. through south; on the contrary, towards the Atlantic coast of North America the direction was N.E. At the same time I observed in this memoir (p. 599.), that the greater number of hurricanes in the southern hemisphere, which I had examined, are whirlwinds rotating in the opposite direction.

Three years later, Mr. Redfield of New York, arrived at the same result by independent observations, as appears from his memoir, entitled "Remarks on the prevailing Storms of the Atlantic Coast of the North American States." (Silliman's Amer. Journ. of Sc., 1831. Avril.) In a later memoir, however, on the gales and hurricanes of the western Atlantic (*ib.* vol. xxxi.), Mr. Redfield has added a new and weighty fact to those already accumulated by me. From the storms, the course of which the American philosopher has there discussed and delineated by a chart, it follows, that the hurricanes taking their rise within the tropics, so long as they are confined between these limits, retain unaltered their original direction from S.E. to N.W.; so soon, however, as they reach the temperate zones, they suddenly bend round almost at a right angle and then travel from S.W. to N.E. Finally, Lieut.-Colonel Reid, in his valuable treatise "On the Law of Storms," published in 1838, confirms by new examples the results already obtained, and especially calls attention to the fact, that by this change in the direction of its course the whirlwind spreads itself out continually from the centre more and more.

So long ago as the year 1735, Hadley proposed to solve the problem of the trade-winds, upon the principle, that air moving from the equator to the poles gradually acquires a westerly, and on the contrary air moving from the poles to the equator an easterly direction. A simple modification, or rather extension of this theory, gives a key to all the complicated phenomena of the variable and apparently so irregular motions of the wind observed in our own and other extra-tropical latitudes. It is only necessary to take into consideration the two currents contending for and alternately obtaining the upper hand, in order to see that the origin of the current, which Hadley treated as fixed for a given place, is in fact variable; wherefrom it follows, that the direction of the vane ought to be not stationary, but changeable, according to a law which I have named the law of rotation. From the same

fruitful principle I am now prepared to explain theoretically the phenomena of storms.

Let ab be a series of material points parallel to the equator, which are set in motion by a certain impulse in the direction at from south to north. The rotation of the earth combined with this impulse will produce a motion of ab towards gh , if the space dbh is void of matter. But if this space is filled by quiescent matter, the particles at b will, as they move, come into contact with particles in the space dbh , which rotate with less velocity; their motion in the direction of east will therefore be retarded, and the point b will move not towards h but towards f . The particles at a are, on the contrary, in juxta-



position with particles, which at first have an equal velocity of rotation, and consequently move as they would in a vacuum, that is, towards g . If, then, ab represent a mass of air impelled from south to north, the storm will have a more southerly direction at the east side, a more westerly one on the west side, and will thus acquire a tendency to whirl in the direction S.E. N.W. This tendency to whirl would not take place, were there no resisting matter in the space dbh , and will therefore increase in proportion as this resistance prevents the course of the storm from deviating towards the west. Now within the north tropical regions the space dbh is filled with air, which flows from N.E. to S.W. Here, therefore, the resistance is at a maximum, and the air at b has its westerly tendency so far checked, that it retains its original direction towards d almost unaltered, whilst, on the other hand, the air at a has acquired a tendency to move towards d . The storm accordingly will whirl with the greatest intensity, but retains its initial direction and lateral magnitude. So soon, however, as it reaches the temperate zone, it finds itself in contact with air at dbh , which is in motion from S.W. to N.E. The resistance, which the particles at b experience, will therefore be considerably diminished, or even almost vanish, that is to say, the direction bd is transformed into the direction bh , and the storm bends round almost at a right

angle, and at the time grows wider and wider as it progresses.

The phænomena of storms south of the equator may easily be inferred from what has been said above of the opposite hemisphere. The rectilinear course within the tropics, the sudden curvature at the limits of the tropics and the temperate zones, the accompanying expansion of the whirls constituting the storm; in a word, all the essential phænomena of storms, must clearly be the same for one hemisphere as for the other, with the sole exception, that in the one (the northern) the rotation is after the order of the letters S. E. N. W., and in the other (the southern) after the order of the letters S. W. N. E.

I take this opportunity to make, in my own vindication, a few remarks upon the manner in which my labours in this field have been brought under the notice of the English public. In the Lond. and Edinb. Phil. Mag., vol. xi. p. 390, a paper by Mr. Dalton has appeared, in which I am directly charged with claiming for myself a theory which he had already many years before made known, and which had still earlier been promulgated by the celebrated Hadley. Upon a diligent perusal of Mr. Dalton's Meteorological Essays and Observations, and of Hadley's original Memoir (The Cause of the general Trade-wind, Phil. Trans. 1735, p. 58), I have not succeeded in finding a single trace of, or bare allusion to, the existence of the law of rotation, which it was the principal object of my paper to establish by observations, and explain upon theoretical principles. And if your readers or Mr. Dalton should do me the honour to look into my Meteorological Essays, Berlin, 1837-38, pp. 244-250, it will be seen, that so far from attempting to usurp the credit so justly due to Hadley for the fundamental idea, upon which my own theory is founded, and which he had himself so successfully applied to the particular problem of the trade-winds, I have been anxious to acknowledge the full extent of my obligations to him, and to bring his merits as a discoverer prominently forward.

In an article upon Lieut.-Colonel Reid's law of storms in the Edinburgh Review, I find my meteorological researches again alluded to, but upon a distinct ground. The anonymous Reviewer, in his patriotic anxiety to satisfy his readers of the purely British growth of this theory, allows that some remarkable passages upon the subject had previously appeared in the memoirs of the Berlin Professor, but that these are mere ingenious speculations, *for they are no more*. The term *passage*, for a memoir (on barometric minima) of seven-

Phil. Mag. S. 3. Vol. 17. No. 111. Nov. 1840. 2 B

teen closely printed pages, strikes me as a little extraordinary; but perhaps this arises from my imperfect acquaintance with the nice distinctions of your language. I leave it to my English readers to determine, with what degree of justice results deduced from a greater number of contemporary observations, than (as I believe) had ever previously or have even since been brought together, can be represented as *no more than ingenious speculations*.

I am, dear Sir, yours with much esteem,

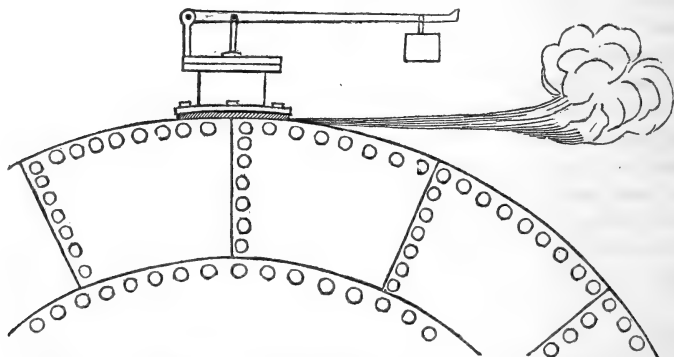
Berlin, Sept. 30, 1840.

H. W. DOVE.

LV. *On the Electricity of a Jet of Steam issuing from a Boiler.*
By H. G. ARMSTRONG, Esq., in *Letters to Professor Faraday**.

SIR,

A FEW days ago, I was informed that a very extraordinary electrical phænomenon, connected with the efflux of steam from the safety-valve of a steam-engine boiler, had been observed at Seghill, about six miles from Newcastle. I therefore took an early opportunity of going over to that place, to investigate the truth of what I had heard, and by so doing I have ascertained the precise facts of the case, which appear to me to possess so much novelty and importance, that I deem it right to transmit the particulars to you, believing that in your hands they will prove most conducive to the advancement of science. Without further preface, I shall proceed to narrate what I saw and heard on the spot.



There is nothing remarkable in the construction of the boiler, which is supported upon brick-masonry in the usual way. The annexed sketch represents an end view of the

* Communicated by Professor Faraday.

boiler and safety-valve, by which it will be seen that the valve is placed on the top of a small cylinder, having a flange round the lower end, which is fastened by bolts to the summit of the boiler, between which and the flange, a cement, composed of chalk, oil and tow, is interposed for the purpose of making the joining steam-tight.

About three weeks ago the steam began to escape at this joining, through a fissure in the cement, and has ever since continued to issue from the aperture in a copious horizontal jet. Soon after this took place, the engine-man, having one of his hands accidentally immersed in the issuing steam, presented the other to the lever of the valve, with the view of adjusting the weight, when he was greatly surprised by the appearance of a brilliant spark, which passed between the lever and his hand, and was accompanied by a violent wrench in his arms, wholly unlike what he had ever experienced before. The same effect was repeated when he attempted to touch any part of the boiler, or any iron-work connected with it, provided his other hand was exposed to the steam. He next found that while he held one hand in the jet of steam, he communicated a shock to every person whom he touched with the other, whether such person were in contact with the boiler, or merely standing on the brick-work which supports it; but that a person touching the boiler, received a much stronger shock than one who merely stood on the bricks.

These singular effects were witnessed and experienced by a great many persons, and among others by two gentlemen with whom I am personally acquainted, and who fully corroborate the above account, which I obtained from the engine-man.

The boiler had been cleaned out the day before I saw it, and a thin incrustation of calcareous matter reaching as high as the water level had been removed, and the consequence was, that the indications of electricity, though still existing, were very much diminished. Still, however, what remained was very extraordinary; for when I placed one hand in the jet of steam and advanced the other within a small distance of the boiler, a distinct spark appeared, and was attended with a slight electrical shock.

From the effect produced by the cleaning of the boiler, it appears pretty obvious that the phænomenon is in a great measure, though not wholly, dependent upon the existence of an incrustation within; and the reason why such effects do not in any degree attend the effluxion of a jet of steam from a boiler in ordinary cases, must, I apprehend, be sought for in the fact, that in the present instance the steam escapes

through an aperture in a non-conducting material, while in a vast majority of cases the escape must take place through a metallic orifice. Can the explosion of boilers, respecting the cause of which so much uncertainty at present exists, have any connexion with the rapid production of electricity which thus appears to accompany the generation of steam?

In the present case the incrustation in the boiler is very rapidly formed, and I therefore expect that in a few days the effects will have become as strong as they were at first. Whenever this takes place I shall again go over to witness them, and if you wish for any further information, I shall be glad to obtain it for you. In the mean time you are at liberty to make any use of this letter that you think fit.

I am, Sir, very respectfully yours,

Newcastle-upon-Tyne, Oct. 14, 1840.

H. G. ARMSTRONG.

Newcastle-upon-Tyne, Oct. 22, 1840.

Dear Sir,—I yesterday revisited the boiler at Seghill, in company with some friends, and took with me such apparatus as I deemed necessary for experimenting on the electrical steam. The results of this second visit I now hasten to communicate, and you will find in the following account of my proceedings, answers to all the queries you were kind enough to send me, for the purpose of directing my attention to the proper points of inquiry.

I found the boiler, and everything connected with it, precisely in the state in which I have already described it, and on trying the steam in the same way as I did on the former occasion, the effect was very nearly the same; but when I placed myself on an insulating stool, the intensity of the sparks which passed between my hand and the boiler was greatly increased, as well as the twitching sensation in the knuckles and wrist, which accompanied the operation, and which in my former letter I designated a slight electrical shock. In pursuance of your instructions, I had provided myself with a brass plate, having a copper wire attached to it, which terminated in a round brass knob. When this plate was held in the steam by means of an insulated handle, and the brass knob brought within about a quarter of an inch from the boiler, the number of sparks which passed in a minute was from sixty to seventy, as nearly as we could count; and when the knob was advanced about one-sixteenth of an inch nearer to the boiler, the stream of electricity became quite continuous. The greatest distance between the knob and the boiler, at which a spark would pass from one to

the other, was fully an inch. A Florence flask, coated with brass filings on both surfaces, was charged to such a degree with the sparks from the knob, as to cause a spontaneous discharge through the glass; and several robust men received a severe shock from a small Leyden jar charged by the same process. The strength of the sparks was quite as great when the knob was presented to any conductor communicating with the ground, as when it was held to the boiler. It appeared to make very little difference in what part of the jet the plate attached to the conducting wire was held; but when a thick iron wire was substituted for the plate, the effect was greatest when the wire was held very near to the orifice. The valve was loaded at the rate of thirty-five pounds per square inch; but the pressure of the steam fluctuated considerably, which gave me an opportunity of observing that the quantity of electricity derived from the jet increased and diminished with the pressure. The electricity of the steam was *positive*; for when the pith balls of the electrometer diverged upon an instrument connected with the steam, they were attracted by a piece of sealing-wax rubbed on woollen cloth; and when a pointed wire was held by the person on the stool, under the shade of a hat, *a pencil*, and not *a star*, of electrical light became visible.

Besides the principal jet of steam which I operated upon, there were several small streams issuing from different parts of the boiler, and in each of these the electrometer indicated the presence of electricity. From the peculiar manner in which the steam blew off from the safety-valve when the weight on the lever was lifted, it was quite impossible to try any satisfactory experiment upon the steam which was allowed to escape by that means. I applied the gold-leaf electrometer to various parts of the boiler, which, I ought to observe, is in direct communication with the ground by means of the steam-pipes, but could scarcely detect a trace of electricity in any part of it.

The engine has another boiler besides the one in question, and the two boilers lie immediately adjacent to each other. Having been informed that similar phænomena had been discovered in this second boiler, I proceeded to apply the electrometer to some small pencils of steam which were escaping in different parts, and found the same indications which I had observed under similar circumstances in the first boiler. I then raised the safety-valve, and the column of steam which escaped from it proved as highly charged with electricity as the horizontal jet which issued from the other boiler, and in which the phænomenon had first been observed.

Upon inquiry, I found that the water used in the boilers was obtained from a neighbouring colliery, where it was pumped out of the mine, and that the same water was used for the boiler of a small high-pressure engine adjoining the colliery from which the water was procured. In order, therefore, to form an opinion whether or not the phænomenon in question was dependent upon the quality of the water from which the steam was generated, I proceeded to examine the steam evolved from the boiler to which I had been referred, and which proved to be a very small one. The valve was loaded with only twenty pounds on the square inch, and I learned from the engine-man that no appearance of electricity had ever been noticed in the steam. Upon trial, however, I succeeded in obtaining very distinct sparks of electricity from the column of steam which issued from the safety-valve. The sparks were certainly weaker than those obtained at the other engine, but this may reasonably be ascribed to the inferior pressure of the steam, and smaller size of the boiler.

I then repaired to another high-pressure engine, which belonged to the same establishment, and the boiler of which was supplied with *rain* water instead of that drawn from the mine. In this case the pressure of the steam was forty pounds on the square inch. The valve was inaccessible, but a powerful jet of steam was obtained from the upper gauge-cock; I could not, however, obtain any trace of electricity in the steam from this boiler, not even sufficient sensibly to affect the gold-leaf electrometer. The presumption, then, is exceedingly strong, that the phænomenon is in some way occasioned by the peculiar nature of the water from which the steam is produced. I inclose you a specimen of the incrustation*, of a month's growth, deposited by the water from the mine in the boilers in which it is used.

I shall be glad to receive any further instructions from you as to the proper mode of pursuing the investigation, and should be much gratified to hear your opinion as to the cause of this most curious phænomenon †.

I am, dear sir,

Very respectfully yours,

H. G. ARMSTRONG.

M. Faraday, Esq.

* The incrustation is grey and hard; it contains traces of a soluble muriate and sulphate, but consists almost entirely of sulphate of lime, with a little oxide of iron and insoluble clayey matter, carried in probably by the water. There is hardly a trace of carbonate of lime in it.—M. F.

† The evolution of electricity by vaporization, described by Mr. Armstrong, is most likely the same as that already known to philosophers on

LVI. *Experiments on the Electricity of High-Pressure Steam.*
By H. L. PATTINSON, Esq., F.G.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

A VERY singular phænomenon, viz. the production of electricity by two steam-boilers, has been observed in this neighbourhood within the last few weeks, the particulars of which I have the pleasure of transmitting to you for publication in your valuable Journal. The boilers in question are situated at Cramlington Colliery, eight miles north-east of Newcastle, where they supply steam to a high-pressure engine of 28-horse power, employed on the waggon-way to haul full and empty waggons to the top of two inclined planes, leading to the Colliery on the one hand, and to the river Tyne on the other. The boilers are cylindrical, with circular ends, each twenty-one feet long, and five feet diameter. They are supplied with water from an adjacent pond by iron feed-pipes, four inches diameter, and the steam they produce is conveyed to the working cylinder by other iron pipes, six inches diameter, which pipes form also a direct metallic communication between the tops of the boilers. By means of appropriate valves the steam is supplied to the cylinder from one or other boiler at pleasure. A pipe, two inches diameter, leads from the bottom of one boiler on the outside of the brick-work to the ash-pit, through which the sediment deposited by the water is occasionally blown from one of Scott's patent collecting cones, and a similar pipe is attached to the other boiler. The boilers are set in brick-work in the usual way, the fires below, with flues reaching all round, and passing into the chimney also in the usual manner. The flues are covered with large flat bricks, and in the space between the boilers the two flues are necessarily separated by a brick wall. The safety-valves are attached to the boilers by flange joints; and between the flanges, to render them steam-tight, is placed a ring of plaited hemp covered with a cement of litharge, sand and linseed oil, mixed up together, and when applied of the consistence of glaziers' putty. This cement, as it soon becomes hard, is used about the engine for steam joints which occasionally fail; but all the joints of the pipes are made of iron borings and

a much smaller scale, and about which there are as yet doubts whether it is to be referred to mere evaporation, as Harris says, or to chemical action, according to others. This point it neither settles nor illustrates; but it gives us the evolution of electricity during the conversion of water into vapour, upon an enormous scale, and therefore brings us much nearer to the electric phænomena of volcanos, water-spouts and thunder-storms, than before.—M. F.

sal-ammoniac, as ordinarily employed by engine-wrights. The steam is worked at a pressure of thirty-five pounds per inch.

The joint between the top of one of the boilers and the seat of its safety-valve had given way, and steam was issuing forcibly through this aperture, when on Tuesday, September 29th last, the engine-man, William Patterson, while standing with this current of steam blowing upon his legs, took hold of the weight attached to the lever of the safety-valve, to try the strength of the steam, when he felt a peculiar pricking sensation in the ends of his fingers, but as the steam prevented him from seeing distinctly, he thought he had merely struck his fingers rather suddenly against the weight. On Friday, October 2nd, on taking hold of the lever, he again felt a sensation in his fingers of the same kind as before; and on Saturday, the 3rd, on touching the weight, this sensation was stronger, and more distinct; so much so, as to arrest his attention and lead him to mention it to some other workmen employed about the engine, who all handled the weight, and convinced themselves that there was something about it very unusual. During the time they were thus employed, Patterson applied his finger gently to the lever, and perceived a spark. This was repeated by the whole party, and they soon found that sparks could be obtained from any part of the end of the boiler, as far as the valve upon the steam-pipe connecting the two boilers, and also from the pipe through which the sediment is blown, as already described. They observed further, that while standing in the volume of steam issuing from the joint, and touching the boiler, these sparks were always much stronger than when the boiler was touched by a person not in the current of steam. In one or two cases, according to their account, when the current of steam issuing from the joint was very strong, the person exposed to it being probably partially insulated by standing upon the dry and warm brick-work surrounding the boiler, gave strong sparks to others out of the current on bringing his hands to theirs; and once or twice they felt, under these circumstances, something like a slight electrical shock. It may be observed, that at this time the weather was exceedingly fine and dry. It was not long before the engineer of the colliery, Mr. Marshall, became acquainted with these circumstances, and his first feeling was to apprehend that the boiler was in danger of exploding, for, as he said, "when there was fire on the outside of the boiler, he did not know what there might be within." He accordingly sent to Messrs. Hawks's, of Gateshead, who built the boiler, for a person to examine it, and Mr. Golightly, their manager in that department, went out on Wednesday, the 7th inst., for that purpose. He gave his opinion as to the safety of the boiler, and

returned much surprised at the phænomena it presented. The singular circumstance of a steam-boiler yielding electrical sparks, and giving shocks, now began to be noised abroad; and my friend, Mr. Henry Smith, of Newcastle, who had heard the account both from Mr. Golightly and Mr. Marshall, wrote me a note acquainting me with the matter, and desiring me to go with him to see it, which I did on the 11th inst., and again on the following day, having with us the second time proper electrical apparatus. On our first visit, the boilers being unplugged and empty, we merely satisfied ourselves as to all the particulars of their setting, etc., already detailed. Next day, on our arrival, we found the engine at work, the steam up to a pressure of thirty-five pounds an inch, and blowing off strongly at the joint in the boiler. The day was a little damp, but yet not unfavourable, and we were informed on alighting that the indications of electricity were very faint and weak; however, we proceeded to our examination, of which the following is the result.

1. On touching the boiler with the blunt point of a penknife anywhere about the circular end, the weight or the safety-valve itself, with the steam strongly blowing out of the joint, but with no part of the person exposed to the volume of steam, no spark could be perceived whatever.

2. On immersing one hand in the current of steam, and touching the parts of the boiler already named with the point of a penknife held in the other, a very minute but distinct spark was perceived, and this occurred equally on all parts of the boiler, or safety-valve, within reach.

3. By standing in the current of steam, so as to allow it to blow forcibly upon the person, the spark became larger; it was then one-eighth of an inch long.

4. On holding a large shovel in the current of steam with one hand, and touching the boiler with a penknife held in the other, a spark was obtained three-eighths of an inch long.

5. The cap of a gold-leaf electrometer, the bottom of which was held in the hand, was applied to the weight, the body of the operator being entirely out of the current of steam; and no divergence was produced whatever.

6. The electrometer held in the hand had its cap applied to the weight, the other hand of the operator being immersed in the current of steam: strong divergence was immediately produced.

From this it was evident that the electricity proceeded from the steam; but as the boiler-house was damp, so that insulation by glass could not well be preserved, a copper wire was attached to the shovel already mentioned, the end of which wire terminated in the engine-house, some yards distant from the

boiler-house, where was placed a table. The shovel was held by Mr. Smith in the current of steam, with its edge about an inch and a half from the aperture through which the steam issued, and the wire leading away from the shovel was insulated by being attached to sticks of sealing-wax held by assistants. Mr. Smith stood on an insulating stool.

7. On touching a pith-ball electrometer, the threads of which were five inches long, with the insulated wire leading from the shovel held as mentioned, the balls diverged four inches with positive electricity.

8. The wire was attached to an insulated tin conductor, when it yielded sparks half an inch in length.

9. A pointed wire attached to this conductor exhibited the brush of light a quarter of an inch long, which always attends the escape of positive electricity from a point into the air.

10. A small jar was now charged so strongly as to give a rather disagreeable shock. By this time a large crowd of men, women and boys from the "Pit Raw," or pitmen's residences near the colliery, attracted by the novelty and singularity of the circumstances, had gathered about us, filling the engine-house and looking on with great curiosity and interest. A circle of sixteen of these men and women was formed, and they received together, much to their surprise and merriment, a powerful shock from the charged jar. This was several times repeated, the numbers receiving the shock varying each time from twelve to twenty.

11. A stout card was perforated by a discharge of the jar; and cotton wrapped round the end of a copper wire and dipped in pounded resin, readily set on fire.

12. When the edge of the shovel was made to approach the aperture through which the steam issued as near as three-quarters of an inch, very vivid and bright sparks of that length passed continually between it and the boiler.

13. The second boiler did not discharge steam through any fissure, but on lifting its valve by the hand it blew off in a strong current. When the shovel was held in one hand in this current of steam issuing from the safety-valve, and the boiler was touched with a penknife held in the other, a spark passed exactly, as under the same circumstances in the boiler subjected to the above experiments.

From this it would appear that the steam of both boilers was in the same electrical condition.

During the whole of these experiments the engine was doing its work as usual, occasionally going and occasionally standing; but no difference was observed in the electricity given off by the steam.

I have been most careful to supply an exact account of the

facts of this extraordinary, and, as far as I know, unprecedented case, but I do not offer any theory to account for the phænomena. It is hardly possible to suppose that there is any local peculiarity about these boilers, or the place where they are situated, to occasion the highly electrical condition of the steam produced in them; and yet it is as difficult to suppose the fact of high-pressure-steam being electrical, a general one; for if it were so, it could hardly, up to this time, have escaped observation. The conditions, therefore, under which steam becomes electrical require to be investigated, and it is not unlikely that the investigation may lead to important results.

I am, Gentlemen,

Your obedient Servant,

Bentham-Grove, Gateshead,
October 19, 1840.

H. L. PATTINSON.

LVII. *Note on Elimination.* By J. J. SYLVESTER, F.R.S.,
Professor of Natural Philosophy in University College, London.*

THE object of this brief note is to generalize Theorem 2. in my paper on Elimination which appeared in the last December Number of this Magazine. The Theorem so generalized presents a symmetry which before was wanting. Here, as in so many other instances, the whole occupies in the memory a *less* space than the part.

To avoid the ill-looking and slippery negative symbols, I warn my reader that I now use two rows of quantities written one over the other, to denote the product of the terms resulting from *taking away* each quantity in the under from each in the upper row.

Let $h_1 \ h_2 \ \dots \ h_n$ be the roots of one equation of co-existence.

$k_1 \ k_2 \ \dots \ k_m$ of the other.

And let the prime derivative of the degree r be required. Take *any* two integer numbers p and q , such that $p + q = r$. The derivative in question may be written,

$$\Sigma \left(\frac{\left(\frac{x-h_1}{x-k_1} \cdot \frac{x-h_2}{x-k_2} \cdot \dots \cdot \frac{x-h_p}{x-k_p} \right) \left(\frac{h_1 h_2 \dots h_p}{k_1 k_2 \dots k_q} \right) \times \left(\frac{h_{p+1} h_{p+2} \dots h_n}{k_{q+1} k_{q+2} \dots k_m} \right)}{\left(\frac{h_1 h_2 \dots h_p}{h_{p+1} h_{p+2} \dots h_n} \right) \times \left(\frac{k_1 k_2 \dots k_q}{k_{q+1} k_{q+2} \dots k_m} \right)} \right)$$

N.B. Whatever p and q be taken, so long only as $p + q$

* Communicated by the Author.

= r , the above expression changes nothing but its sign; which, therefore, upon transcendental grounds, it is easy to see is of one name or another, according as p is odd or even.

In the original paper, I asserted this theorem only for the case of $p = 0$ or $q = 0$.

University College, London, Oct. 29, 1840.

LVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 18, **T**HE following letter was read from G. G. Anson, Esq., 1840. addressed to the President, enclosing a specimen of a deposit with which nine acres of land near Exeter, belonging to Lord Radnor, had been covered after the subsidence of a flood, and which was sent by H.R.H. Prince Albert, F.R.S. :—

“Buckingham Palace, June 8, 1840.

“MY DEAR LORD,

“His Royal Highness Prince Albert has commanded me to forward to you the enclosed specimen, which has been sent up to His Royal Highness from Lord Radnor’s place near Exeter, where nine acres of land were covered with this curious substance after a flood had subsided. His Royal Highness thinks it very probable that the subject may already have been brought before the Royal Society, but in case it should not have been, he sends the accompanying packet. It is said that a good deal of it has been applied to the purpose of making waistcoats for poor people.

“Believe me,

“My dear Lord,

“Yours very faithfully,

“G. G. ANSON.”

“*The Marquis of Northampton, President of the Royal Society.*”

The following description of the specimen referred to in the letter, drawn up by John Lindley, Ph. D. F.R.S., was also read :

“Description of the Specimen referred to in the preceding letter.”
By John Lindley, Ph. D., F.R.S.

The plant which overran Lord Radnor’s land is the *Conferva crispa* of Dillwyn, which is said to be the *Conferva fluviatilis* of Linnæus. The species inhabits fresh water, and multiplies with great rapidity, forming entangled strata. The green portion is the *Conferva* in its young state, the white portion is the plant old and bleached. The whole mass consists of articulated filaments, among which are fragments of grass-leaves.

The following papers were then read, or their titles announced :—

1. An Account of Experiments on the Reflecting Telescope. By the Right Hon. Lord Oxmantown, F.R.S.

This paper enters minutely into the details of the experiments, of the precautions requisite to ensure success, and of the manipulations

ultimately adopted in forming a speculum three feet in diameter, subsequently applied to a telescope, mounted in a manner very similar to that of Sir John Herschel. The author states, as the results he arrived at, that specula can be made to act effectively, when cast of the finest speculum metal, in separate portions, and retained in their positions by an alloy of zinc and copper, as easily wrought as common brass, and that they can be executed in this manner of any required size; that castings of the finest speculum metal can be executed of large dimensions, perfect, and not very liable to break; that machinery can be employed with the greatest advantage in grinding and polishing specula; that to obtain the finest polish, it is not necessary that the speculum should become warm, and that any temperature may be fixed upon, and preserved uniform during the whole process; and that large specula can be polished as accurately as small ones, and be supported so as to be secured from flexure.

2. On the theoretical explanation of an apparently new Polarity in Light. By G. B. Airy, Esq., M.A., F.R.S., Astronomer Royal.

The existence of a polarity in the rays of homogeneous light, having regard only to the sequence of colours in the spectrum, was inferred by Sir David Brewster from some experiments, of which he has given an account, contained in the Report of the Seventh Meeting of the British Association. The author states the results of his own observations of similar phænomena, and their theoretical explanation on the undulatory theory, together with the mathematical development of that explanation*.

3. On the Ferrosesquicyanuret of Potassium. By Alfred Smee, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

The author examines, in this paper, the action of chlorine upon the ferrocyanate of potassa, and the conversion of the latter into ferrosesquicyanuret; and proposes methods for obtaining this latter salt uncontaminated with impurities, and free from the difficulties and inconvenience attendant on the present mode of preparation †.

4. On the influence of Iodine in rendering several argentine compounds, spread on paper, sensitive to light; and on a new Method of producing, with greater distinctness, the Photogenic Image. By Mr. Robert Hunt. Communicated by Sir John Herschel, Bart., V.P.R.S.

This paper contains various details of the results of a great number of experiments made with a view of rendering paper capable of being employed instead of metallic plates, in Daguerre's photographic process. It is accompanied with 12 papers as specimens.

5. Hourly Observations of the Barometer and Thermometer at sea, on the 21st of March, 1840. By Major-General A. Lindsay, H.E.I.C.S. Communicated by Sir John F. W. Herschel, Bart., V.P.R.S.

These observations were made on board the ship *Owen Glendower*, on her voyage from Calcutta to London.

* See our present volume, p. 81.—EDIT.

† Mr. Smee's paper will be found in our present volume.

6. On the Constitution of Pigotite, and on the Mudesous and Mudesic Acids. By James F. W. Johnston, Esq., M.A., F.R.S.

In this paper the author describes a substance, found by himself and by the Rev. M. Pigot, forming an incrustation on the sides of certain caves, occurring in the granitic cliffs on the east and west coast of Cornwall. This incrustation is in mass of a brown, and in powder of a yellow colour; is insoluble in water and alcohol; when heated, it gives off much water, blackens, yields empyreumatic products, and leaves a black mass, having occasionally the lustre of graphite. In the air, at a bright red heat, this mass very slowly burns, leaving a grey or white ash, which consists of alumina, with some slight foreign admixtures.

The organic constituent of this substance (pigotite), the author considers to be derived from the decay of the various plants which grow on the moist moorlands above, and which, being carried by the waters into the fissures of the granite beneath, combines with the alumina of the decomposed felspar; and when it reaches the air, deposits itself on the roof and sides of the caverns, in the form of layers, varying from a line to two or three inches in thickness. With reference to its supposed origin, the author has given to the organic constituent the name of *mudesous* acid (from *μυδῆσις*, signifying decay through excess of moisture); and he mentions an observation, communicated to him by Dr. Boase, that the roots of the sea-pink (*Statice Armeria*) contain a colouring matter resembling, in appearance, the solutions of the mudesous acid.

From numerous experiments and analyses detailed at length in his paper, the author derives the following general results:

1. That the native pigotite contains a dark-brown soluble, not deliquescent acid of vegetable origin, which, in the anhydrous state, is represented by $C_{12}H_5O_8$.

2. That this acid, the *mudesous*, is tribasic, the salt of silver (mudesite), being represented by $(3AlO + C_{12}H_5O_8)$, and precipitates the salts of the metallic oxides of a brown colour.

3. That the native mudesite of alumina (Pigotite) is represented as follows:

a. Dried in the air by $(4\overline{Al} + C_{12}H_5O_8 + 27HO)$.

b. Dried at 212° F. by $(4\overline{Al} + C_{12}H_5O_8 + 8HO)$, losing 27 per cent. of water.

c. Dried at 300° F. by $(4\overline{Al} + C_{12}H_5O_8 + 8HO)$, losing 32 per cent. of water.

4. That this native mudesite, however, is more probably a compound of the organic tribasic salt, with a hydrate of alumina, and may be rationally represented thus:

a. Dried in the air by $(\overline{Al} + C_{12}H_5O_8 + 9HO) + 3(\overline{Al} + 6HO)$.

b. Dried at 212° F. by $(\overline{Al} + C_{12}H_5O_8 + 4HO) + 3(\overline{Al} + 2HO)$.

c. Dried at 300° F. by $(\overline{Al} + C_{12}H_5O_8 + 2HO) + 3(\overline{Al} + 2HO)$.

5. That when treated with nitric acid, the native mudesite, as well as the mudesous acid itself, are oxidized and converted into a new brownish-yellow, soluble and deliquescent acid, containing more oxygen, and in the anhydrous state represented by $C_{12}H_5O_{10}$.

6. That this new acid, the *mudesic*, combines readily with alumina and protoxide of mercury, giving salts of a yellow colour. Both the acids described in this paper are distinguished for their tendency to precipitate alumina and the protoxide of mercury. The mudesate of mercury dried at $300^\circ F.$, is represented by $(2H_9O + C_{12}H_5O_{10})$.

7. That chlorine, when made to act on either of the acids, or their salts of alumina in contact with water, gradually deprives them of all colour, while, at the same time, muriatic acid is formed. Collected on the filter, boiled in water till the washings cease to precipitate nitrate of silver, and dried, the white gelatinous, apparently altered mudesite or mudesate, is found on analysis to contain no atomic proportion of chlorine, but to have sensibly the constitution of the mudesic acid, or mudesates prepared by the direct action of nitric acid. The author thinks it not unlikely that a chloro-mudesic acid exists, and may be formed during this process, represented probably by $C_{12}H_4ClO_{10}$, but which he has not succeeded in obtaining in a separate state.

The mudesous and mudesic acids are distinguished from each other by giving, the former brown, and the latter yellow precipitates with the neutral metallic salts—by being, the former unaltered, and the latter deliquescent in the air. Both form deliquescent salts with ammonia, and appear to undergo alteration by the long-continued action of hydrosulphuric, or of concentrated sulphuric and hydrofluoric acids.

7. On the Constitution of the Resins, Part V. By James F. W. Johnston, Esq., M.A., F.R.S.*

In this paper the author continues his examination of what are called the fetid resins, and from repeated analyses deduces for the resin of Sagapenum the formula $C_{40}H_{29}O_5$, and for that of Galbanum $C_{40}H_{27}O_7$. He then compares the formulæ for the four resins:

Opoponax = $C_{40}H_{25}O_{14}$, Assafoetida = $C_{40}H_{26}O_{10}$,

Galbanum = $C_{40}H_{27}O_{70}$, Sagapenum = $C_{40}H_{29}O_9$;

and considers it probable that, though no striking analogy among the *irrational* formulæ for these resins is perceptible, by which their analogy in physical properties can be accounted for, they may possess an analogous *rational* constitution which future researches may disclose.

Euphorbium consists of two resins, of which the more soluble, A, gave the formula $C_{40}H_{31}O_6$. Elemi also consists of two resins, of which the more soluble, A, is represented by $C_{40}H_{32}O_4$, and the less soluble, B, by $C_{40}H_{32}O_1$, as had previously been shown by Hess

* Abstracts of the preceding series will be found in Lond. and Edinb. Phil. Mag., vol. xv. p. 327, and present vol. p. 147.—EDIT.

and Rose. The Bdellium of commerce contains much gum, and a resin $C_{40}H_{31}O_5$.

The resin of Benzoin presented peculiar difficulties when submitted to investigation, from the ease with which it undergoes decomposition, even at temperatures much below that at which it melts. With regard to this resin, the author gives the following as the result of his numerous analyses:—

1. That the colourless resin of benzoin is represented approximately by $C_{40}H_{22}O_9$.

2. That by heat and dilute carbonated alkalies it is decomposed into water, benzoic acid, a little volatile oil, and a resin $C_{40}H_{23}O_6$, or $C_{40}H_{24}O_6$.

3. That by boiling with quicklime, or concentrated carbonated alkalies, it gives two resins, one in large quantity = $C_{40}H_{24}O_8$; and another in small quantity = $C_{40}H_{30}O_7$.

4. That by caustic potash the crude resin is resolved into two resins represented respectively by $C_{40}H_{22}O_9$, and $C_{40}H_{30}O_7$, of which the former is precipitated, and the latter remains in solution, when a saturated aqueous solution of caustic potash is added to an alcoholic solution of the crude resin.

5. And that by oxide of lead two resins are separated, for which analysis gave respectively the formulæ $C_{40}H_{22}O_9$, and $C_{40}H_{26}O_{10}$.

The author concludes by stating that such metamorphoses are by no means confined to this resin, though the more accurate knowledge of their nature, obtained by the imperfect study he has made of the resin of benzoin, has explained many anomalies he had previously observed, with regard to the relations of the resins to the alkalies and metallic oxides. He considers the group of which dragon's blood is the type, and which he represents by the expression $C_{40}H_{24} \pm xO_9$ to be peculiarly susceptible of modification (or decomposition?) by the action of bases; and he specifies among other results, with regard to which it is his intention to address the Society in a future paper, that dragon's blood, of which the lump variety = $C_{40}H_{21}O_8$, and the drop variety (heated to $300^\circ F.$) = $C_{40}H_{20}O_8$, gives by the action of quicklime and oxide of lead, among other products, two resins represented approximately by $C_{40}H_{20}O_{10}$ and $C_{40}H_{20}O_{12}$?—that guaiacum = $C_{40}H_{23}O_{10}$, with oxide of lead, gives a resin = $C_{40}H_{21}O_{11}$, the resin of jalap = $C_{40}H_{34}O_{18}$; by the action of the same oxide, a resin = $C_{40}H_{34}O_{20}$, and that of assafœtida = $C_{40}H_{26}O_{10}$, a new resin = $C_{40}H_{23}O_{13}$. These metamorphoses lead to the second great branch of inquiry respecting the nature and constitution of the resins. Certain results being established, at least approximately, with regard to the *irrational* constitution of the resins, and certain general irrational formulæ by which to express it, we are prepared for the study of their *rational* constitution. This part of the subject the author proposes to consider farther in subsequent communications.

8. Researches on the Tides. Twelfth Series. On the Laws of the Rise and Fall of the Sea's surface during each tide. By the

Rev. W. Whewell, B.D., F.R.S., Fellow of Trinity College, Cambridge*.

The materials of the present investigation are five months' tide observations made at Plymouth; three months' observations made at Liverpool, under the direction of Captain Denham, R.N.; and twelve months' observations made at Bristol, by Mr. Bunt, by means of his tide-gauge. According to the theory of the tides, the height of the surface of the water at a given place will increase as the sine, while the time increases as the arc. Hence if the time be made the abscissa, and the height the ordinate, the curve representing one tide would be the *figure of signs*. The author on making the comparison of the empirical curve of the rise and fall of the water, deduced from observation, with this theoretical curve, finds a general agreement between them; subject to certain deviations, consisting principally in the empirical curve indicating that both the rise and the fall are not symmetrical, like the theoretical curve, in consequence of the fall being generally more rapid than the rise, and thus occasioning a displacement of the summit of the curve towards that branch of it which corresponds to the fall.

9. Researches in Embryology. Third Series.—Additional Observations. By Martin Barry, M.D., F.R.S.†

Having in the paper to which the present is supplementary made known the fact that the germinal spot in the mammiferous ovum resolves itself into cells, with which the germinal vesicle becomes filled, the author has since directed his attention to the corresponding parts in the ova of birds, batrachian reptiles, and osseous fishes, which he finds to be the seat of precisely the same changes. The numerous spots in the germinal vesicle of batrachian reptiles and osseous fishes are no other than the nuclei of cells. The cells themselves, from their transparency, are at first not easily discerned, and appear to have hitherto escaped notice; but after the observer has become aware of their presence, they are, in many instances, seen to be arranged in the same manner, and to present the same interior themselves as the corresponding cells in the ovum of mammalia.

In the representations given by Professor Rudolph Wagner, the discoverer of the germinal spot, the author recognizes evidence of the same changes in ova throughout the animal kingdom. He confirms and explains the observations of R. Wagner, that in the ova of certain animals an originally single spot divides into many, and that in the ova of other animals the number of spots increases as the ovum ripens. But he expresses also the opinion that in all ova there is originally but a single spot, this being the nucleus of the germinal vesicle or cell.

The analogy between the ova of mammalia and the animal above-mentioned, extends also to the substance surrounding the germinal vesicle, which consists of nucleated cells.

10. Description of a Calculating Machine invented by Mr. Thomas

* See Lond. and Edinb. Phil. Mag., vol. xv. p. 316.—EDIT.

† *Ibid.*, vol. xvi. p. 526.—EDIT.

Fowler, of Torrington in Devonshire. By Augustus De Morgan, Esq. Communicated by F. Baily, Esq., V.P.R.S.

The arithmetical operations performed by the machine are those of multiplication and division; the factors and product in the former case, and the quotient, dividend and divisor in the latter, being expressed in digits of the ternary scale of notation, every digit being either -1 , 0 , or $+1$. In this system, unity being, in multiplication, only an index, the rules for multiplication and division must consist entirely in directions for the management of the signs of unity; and it is on this principle that Mr. Fowler's machine is made to act. A short account is given of the principal parts of the machine, and of the mode in which they bring out the final results. It is necessary, however, in applying it to use, to have recourse to tables, both for converting the factors and reconverting the result; operations which introduce both labour and risk of error.

11. On the Minute Structure and Movements of Voluntary Muscles, in a letter addressed to R. B. Todd, M.D., F.R.S., &c. By William Bowman, Esq., Demonstrator of Anatomy in King's College, London, and Assistant Surgeon to King's College Hospital. Communicated by Dr. Todd.

The objects of the author, in this paper, are the following.—1st. To confirm, under some modifications, the view taken of the primitive fasciculi of voluntary muscles being composed of a solid bundle of fibrillæ: 2dly. To describe new parts entering into their composition: and 3dly. To detail new observations on the mechanism of voluntary motion.

He first shows that the primitive fasciculi are not cylindrical, but polygonal threads; their sides being more or less flattened where they are in contact with one another; he next records, in a tabular form, the results of his examination of their size in the different divisions of the animal kingdom. It appears that the largest are met with in fish; they are smaller in reptiles, and their size continues to diminish in insects, in mammalia; and lastly, in birds, where they are the smallest of all. In all these instances, however, an extensive range of size is observable, not only in different species, but in the same animal, and even in the same muscle. He then shows that all the fibrillæ into which a primitive fasciculus may be split, are marked by alternate dark and light points, and that fibrillæ of this description exist throughout the whole thickness of the fasciculus; that the apposition of the segments of contiguous fibrillæ, so marked, must form transverse striæ, and that such transverse striæ do in fact exist throughout the whole interior of the fasciculus. He next inquires into the form of the segments composing the fibrillæ, and shows that their longitudinal adhesion constitutes *fibrillæ*, and their lateral adhesion *discs*, or plates, transverse to the length of the fasciculus; each disc being, therefore, composed of a single segment from every one of the fibrillæ. He shows that these discs always exist quite as unequivocally as the fibrillæ, and gives several examples and figures of a natural cleavage of the fasciculus into such discs. It follows that the transverse striæ are the edges, or focal sections of

these discs. Several varieties in the striæ are then detailed, and the fact noticed that in all animals there is frequently more or less diversity in the number of striæ in a given space, not only on contiguous fasciculi, but also on the same fasciculus at different parts.

The author then proceeds to describe a tubular membranaceous sheath, of the most exquisite delicacy and transparency, investing each fasciculus from end to end, and isolating it from all other parts; this sheath he terms *Sarcolemma*. Its existence and properties are shown by several modes of demonstration; and among others, by a specimen in which it is seen filled with parasitic worms (*Trichinæ*), which have removed all the fibrillæ. The adhesion of this sarcolemma to the outermost fibrillæ is explained.

It is also shown that there exist in all voluntary muscles a number of minute *corpuscles* of definite form, which appear to be identical with, or at least analogous to the nuclei of the cells from which the development of the fasciculi has originally proceeded. These are shown to be analogous to similar bodies in the muscles of organic life, and in other organic structures.

The author next describes his observations on the mode of union between tendon and muscle; that is, on the extremities of the primitive fasciculi. He shows that in fish and insects the tendinous fibrillæ become sometimes directly continuous with the extremities of the fasciculi, which are not taper, but have a perfect terminal disc. In other cases the extremities are shown to be obliquely truncated, where the fasciculi are attached to surfaces not at right angles to their direction.

Lastly. He states his opinion, and gives new facts on which it is founded, that in muscular contraction the discs of the fasciculi become approximated, flattened, and expanded; the fasciculi, of course, at the same time becoming shorter and thicker. He considers that in all contractions these phænomena occur; and he adduces arguments to show the improbability of the existence of any rugæ or zigzags as a condition of contracting fasciculi in the living body. The paper is abundantly illustrated by drawings of microscopic appearances.

The Society then adjourned over the long vacation, to meet again on the 19th of November.

GEOLOGICAL SOCIETY.

[Continued from p. 309.]

MINING RECORDS OFFICE.

A third department, which it is proposed to add to this establishment, is an office, for the preservation of such records and documents relating to subterranean operations throughout the country as are important to be preserved for the information of future generations.

To the keeper of these records will be assigned the duty of arranging the documents which may be transmitted to him from all parts of the kingdom, by any engineers, mineral surveyors, and proprietors of mines and coal works, who may be willing to

send them; particularly maps, sections, and under-ground plans, which will record the state of each mine, when it is abandoned, for the information of those who at a future period may be disposed to bring it again into operation. This office will be accessible to all persons interested in obtaining the information it will afford. To this collection several engineers of most extensive experience in the mines of Newcastle and Cornwall have promised large contributions.

The keeper will make copies of documents of this kind, which proprietors of mines, who cannot conveniently part with the originals, may lend, for the purpose of being preserved in this national collection.

The public importance of such a records office was submitted to the Lords of Her Majesty's Treasury by a Committee of the British Association for the Advancement of Science, assembled at Newcastle in August, 1838; it being notorious that great losses of life and destruction of property have resulted both at Newcastle and in other coal mines throughout the kingdom, from the imperfect preservation of records of the operations previously conducted in them, and that still greater losses will inevitably ensue hereafter, unless advantage be taken of the experience of living engineers and coal proprietors, who are willing to place in a public national repository copies of the documents they possess relating to their respective mines.

In 1834, the attention of the public was called to this subject by Mr. T. Sopwith*, an eminent civil engineer and mine surveyor at Newcastle; and this gentleman is preparing a practical book of instructions on the subject of drawing geological and mining plans, the conducting of subterranean surveys, and examining mineral districts, with a view to the preservation of such information respecting the state of each mine at the period when it may be abandoned, as may be useful when further proceedings are afterwards commenced therein, or in its vicinity.

A museum of œconomic geology, comprehending institutions of this kind, demonstrates, even to the unlearned, the advantages that result from science in its application to the extraction of the treasures which Providence has laid up in the rich storehouses of the interior of the earth; and by exhibiting the results obtained from the elaboration of these materials, by the industry of man, in the workshop and at the forge, will afford a full and satisfactory reply to the question so often raised by persons to whom the value of the truths of pure science and philosophy, pursued for their own sake, are unintelligible,—and by whom everything is appreciated merely according to its immediate subserviency to the acquisition of wealth, or its ministration to the daily necessities or conveniences of human life.

BUILDING-STONE COMMISSION.

Another event which marks increasing attention to the practical importance of geology, is the publication of a Report to the Commis-

* See Sopwith on Isometric Drawing, p. 50, *et seq.*

sioners of Her Majesty's Woods and Forests, from a Commission appointed by the Lords of the Treasury; containing the results of an inquiry into the qualities and durability of the various Building-stones of this country, with a view to the selection of the best material to be employed in erecting the New Houses of Parliament.

The results of this inquiry have been arranged in Tables, which represent the composition, colour, weight, size, cost, durability, &c., of all the most important kinds of stone that have been used in ancient edifices in England; the Commissioners having judiciously appealed to that which is the most severe test of the durability of any stone, viz. the existing condition of the decorated architecture in our most ancient buildings.

The Norman portions of the Church of Southwell, in Nottinghamshire, constructed of magnesian limestone, in the twelfth century, have been found to afford an example of stone which combines strength and durability with applicability to ornamental carved work, in a degree surpassing all other kinds of stone that have been employed in the most ancient fabrics of this country; the sharpest of the mouldings and carved enrichments of that church being throughout in as perfect a state as when first executed. The keep of Koningsburgh Castle, near Doncaster, built also of the magnesian limestone in that vicinity, offers another proof of the durability of certain beds of this formation, exceeding that of any other building-stone in Great Britain, which is equally fit for ornamental purposes. But there are also varieties of magnesian limestone, such as that of which York Cathedral is built, which are in far advanced stages of decay, where they have been used for mouldings and architectural decorations.

The general result of this elaborate inquiry into the durability of the different varieties of magnesian limestone is, that the stone resists decomposition in proportion as it is more perfectly crystalline; a result, the cause of which is further illustrated by the experiments of Professor Daniell, which show that the nearer the magnesian compounds approach to *equivalent* proportions of carbonate of lime and carbonate of magnesia, the more crystalline they are.

No investigation has been made by these Commissioners as to the capabilities of granite, porphyries, and other kinds of stone, which are inapplicable to the decoration of edifices without enormous expense.

The Report is followed by valuable tabular lists of the most remarkable ancient fabrics in England, specifying the materials of which they are constructed, and their various conditions of preservation or decay, as they are respectively built of sandstone, or of oolitic, shelly, or magnesian limestone.

To these are added tables of the chemical analysis, weight, cohesive power, specific gravity, and power of absorbing water, of many of the building stones most largely employed in England*.

* [It may be added, as evincing the strong interest which this Report has excited, that its contents have been specially illustrated, by one of the Com-

I consider this Report as of the highest value, in showing the general advantages which may be derived from connecting scientific knowledge with practical arts; and I trust we shall hear no more of such discreditable and unfounded assertions as, not long ago, passed uncontradicted, at a meeting of an architectural society in London, that Stonehenge is made of statuary marble.

GEOLOGICAL COMMITTEE OF ENGLISH AGRICULTURAL SOCIETY.

The appointment of a Geological Committee, by the English Agricultural Society, at their meeting in Oxford, in July last, shows the sense entertained by that numerous body of landed proprietors, and cultivators of the soil of England, of the important services which may be rendered to them, by the application of geological knowledge to the improvement of the productive capabilities of the land.

It is well known to geologists that an almost unbounded supply of mineral manure may be found in the sub-strata, which in very many districts are composed of ingredients different from those of the surface. So constant are the characters of many of the beds of the geological groups which pass in long and narrow bands from one side of England to the other, that a single experiment, carefully conducted, on any one stratum of each formation, with a view to ameliorate its soil, by an admixture of the ingredients of some other adjacent stratum, will afford an example which may be followed with similar results in distant parts of the kingdom, through which this same stratum passes, in its course across the island.

Experiments, therefore, conducted by the owners and occupiers of land, under the advice of this Geological Committee, aided by the facilities for the analysis of soils now afforded by the laboratory of the Museum of Economic Geology, may shortly enable us to realize at least some share of the success that attended Lavoisier's application of chemistry to agriculture in France*.

SCHOOLS OF CIVIL AND MINING ENGINEERING IN THE UNIVERSITIES OF DURHAM AND LONDON.

The increasing demand for education in practical science has been recently provided for in the University of Durham, by the establishment of a course of instruction in Civil and Mining Engineering, with lectures in the Mathematical sciences, Chemistry, Metallurgy, Mineralogy, Geology, Surveying, Mapping, and Drawing, in addition to Ancient and Modern Languages. To theoretical instruction in

missioners, Mr. Charles Smith, in lectures delivered before the Society for the Encouragement of Arts, &c. and the Institute of British Architects; and by Mr. Brayley, in a Course on the Mineralogy of the Arts, delivered before the Architectural Society, at one of the Friday-Evening Meetings of the members of the Royal Institution, and in two lectures expressly on the results arrived at by the Commissioners, given at the Russell Institution.—*EDIT.*]

* It was said of Lavoisier, that in ten years he doubled the produce of his land in grain, while he quintupled the number of his flocks. No doubt this report is much exaggerated.

such parts of these branches of knowledge as bear more especially on Practical Engineering, are added at Durham occasional surveying excursions, both in the field and underground, conducted by a practical civil engineer. More than thirty young men have, during the last year, been actively engaged in this new department of academical study.*

The locality of Durham, upon the margin of the great Newcastle coal field, and in the vicinity of the lead mines of Alston Moor, and Weardale, is in a peculiar degree favourable for a school of mining and civil engineering; enjoying advantages of position similar to those of the great Saxon school at Freyberg, near the mining districts of the Ertzgebirge and the Hartz.

The University of London also is taking measures to institute examinations of Candidates for certificates of proficiency in Civil Engineering, and the arts and sciences connected with Mining.

In University College, London, courses of preparatory experimental lectures and exercises in Natural Philosophy have, during the last year, been provided for the students in that establishment, who are destined for the Profession of Civil Engineers.

And in King's College, London, a course of lectures in Civil Engineering, and Sciences applied to Arts and Manufactures, is at this time attended by more than fifty students, who have the opportunity of adding practical to theoretical knowledge in a workshop and laboratory established for their use.

SCHOOL OF MINES IN CORNWALL.

Another proof of the direction of public attention to the collateral branches of our science has, within the last twelve months, been afforded by the establishment in Cornwall, of a school for the instruction in Sciences and Arts connected with MINING, of young men who are to be engaged in conducting the important subterranean operations of that county. The want of such a school had been pointed out by Mr. John Taylor, in his Prospectus of a School of Mines in Cornwall, February 7, 1825,† and in his Records of Mining, published in 1829. It has at length been instituted chiefly through the exertions and at the expense of Sir Charles Lemon. This incipient school, and the University of Durham, form almost solitary examples in England, of such scientific establishments as are nearly universal in the mining districts of the Continent. The experiment has begun in Cornwall with Courses of Lectures in Mathematics, Mechanics, Chemistry, and Mineralogy, by three professors; and a course of instruction, by a practical surveyor, in Algebra, Drawing, and the Use of instruments: and during the next year, still further additions are contemplated.

* See Durham University Calendar, 1839, p. 10. [See also a communication on this subject by the Rev. Prof. Chevallier and Prof. Johnston, in Lond. and Edinb. Phil. Mag., vol. xiii. p. 1.—EDIT.]

[† The Prospectus here alluded to will be found in Phil. Mag., First Series, vol. lxvi. p. 137.—EDIT.]

POLYTECHNIC SOCIETY OF CORNWALL.

To the zealous exertions of Sir Charles Lemon, and of many intelligent and active individuals at Falmouth, the county of Cornwall is also indebted for the establishment of a Polytechnic Society, which, during the few years of its existence, has been attended with extraordinary success. One of its chief objects is to encourage, by rewards, the invention and improvement of machinery, of which so large an amount is essential to the working of the mines. Another object is to collect materials for expressing the quantity and value of the mineral and other produce of the county; and to construct tables indicating the diminished longevity, and diseases, which, in a peculiar degree, affect the Cornish miners, and do not prevail amongst those employed in Collieries. It appears, from a paper published in the Sixth Annual Report of this Society (1839), that the average duration of a miner's life is less, by many years, than that of the agricultural labourer in the same district; the apparent causes of this frightful evil being the inevitably imperfect ventilation of many of the veins or lodes in which the miner works; and, partly, the extreme fatigue of ascending from great depths by ladders, instead of being lifted by machinery, as the workmen are from coal pits: these pits also are usually susceptible of more perfect ventilation, than the metalliferous lodes in Cornwall.

The attention of this Society is strenuously directed to the discovery of remedies for these tremendous evils, which affect no fewer than a population of 28,000 persons; that being the proportion of the inhabitants of Cornwall, who are occupied in working the mines.

LOCAL MUSEUMS.

Another circumstance which marks the progressive advancement of public feeling as to the value of geology, is the increasing disposition to form local museums in our provincial towns.

At the meeting of the British Association, at Birmingham, in August last, after a strong expression of opinion, in the Section of Geology, as to the benefit likely to accrue to science from the establishment of Provincial Museums, for the local productions of each neighbourhood, the justness of the suggestion was so fully recognised, that, in the adjacent town of Dudley, before five days had passed, a public museum had arisen from contributions, out of the cabinets of private collectors in that town; presenting to the Association a more perfect assemblage than was ever seen, of the exquisite organic remains found in the limestone of that district, which has long been the classic type of a formation widely and abundantly distributed over the globe.

About this time also a provincial museum was formed at BRADFORD, in a district abounding in splendid examples of the vegetable remains which pervade the Yorkshire coal field; where the extensive collieries now wrought will furnish abundant materials for a collection, destined to illustrate the history of the extinct forms of vegetable life, which have produced the coal.

The museum at LEEDS, also, possesses a valuable collection of fossil

vegetables from the coal field in its neighbourhood; and the WEST RIDING GEOLOGICAL SOCIETY, formed under the auspices of Earl Fitzwilliam, on the plan of holding quarterly meetings at different towns of the Riding in succession, is diffusing a taste for Geology, and affording ground for appreciating its practical importance, to numbers of intelligent persons, whose local occupations, and property in the coal and iron mines, will enable them to enlarge the fossil Flora and Fauna of our country.

ROYAL INSTITUTION OF SOUTH WALES.

From the first Annual Report of the Royal Institution of South Wales, published during the last year; we learn that the Swansea Literary and Philosophical Institution, hitherto supported by the town and neighbourhood, has been expanded, under Royal patronage, to the whole southern division of the Principality; and is now establishing its Museum and Lecture Rooms in a large and commodious edifice in the town of Swansea, under the presidentship of Lewis Weston Dillwyn, Esq.

The position of this Institution, in the midst of a great mining and manufacturing district, is peculiarly favourable for collecting facts illustrative of geological phenomena, more especially those of the Coal formation; and much has already been done by Mr. Logan, to develop, with extreme accuracy and minuteness of detail, the stratigraphical succession of the rocks composing this formation; and to show the number and nature of the events which attended their original deposition, as well as the subsequent derangements that have affected them. Mr. L. W. Dillwyn, also, is attempting a classification of the coal plants of the South Wales Basin; with a view to ascertain, by means of a comparative collection in the Swansea Museum, whether there exists any specific difference between those of the upper and lower beds of the carboniferous series.

BRITISH MUSEUM.

The accessions lately made to the British Museum form another subject, of high importance in our Review of the Geological Proceedings for the past year. At the head of these is the purchase, from Mr. T. Hawkins, of an additional series of the remains of fossil *Saurians* from the Lias formation; which, added to his former collection, already placed in this national repository, present an unrivalled series of species in the extinct families of *Ichthyosaurus* and *Plesiosaurus*, once inhabitants of Britain. Equally important was the acquisition, in a former year, of the unique collection of still more gigantic and not less monstrous Reptiles, from the Wealden formation of Kent and Sussex, obtained by purchase from Dr. Mantell. The possession of these several collections places the Museum, where it ought to stand, at the head of all existing repositories of organic remains, almost exclusively the productions of England; and it is due to his late exertions, whilst Chancellor of the Exchequer, that I should bear this public testimony to the services which Lord Monteagle has rendered to science, by supplying the means

of placing these unrivalled collections in our national repository; where their constant presentation to the view of its thousands of daily visitors cannot fail to attract increasing attention to the wonderful discoveries of Palæontology.

These important public events, occurring beyond our walls, and having a direct and immediate tendency to enlarge the field of our labours, form an epoch in the history of our science, and place Geology before the country in a new and more widely popular aspect than it had occupied before. The past year has been also distinguished beyond all precedent, by the number and value of the GEOLOGICAL MAPS it has produced.

GEOLOGICAL MAP OF CORNWALL AND DEVON.

The first map which I shall mention, affords another example of the recognition by Government of the importance of our subject, by their having attached a geological department to the Ordnance Survey of England and Wales. The first fruits of this appointment are the splendid Maps of Devon and Cornwall, and a part of Somerset, coloured after the surveys of Mr. De la Beche; and it may be truly said of them, that they are more beautiful in their execution, more accurate in their details, and more instructive in the æconomical and scientific information they give respecting mines, than any maps yet published by any government in the world; affording documents to which we can at length with pride appeal, in reply to the reproach that has so long, with too much truth, been cast upon us, that England alone, of all the civilized nations, has abandoned to gratuitous individual exertions, and the liberality of amateurs in science, the great work of exploring and delineating the mineral structure of the country; and ascertaining the nature and extent of the subterraneous produce, which lies at the foundation of the industry of its manufacturing population, and to which the nation owes no small portion of its wealth.

The statistical importance of this first portion of the Ordnance Geological Map of England will be duly appreciated only by those, who know the extent of the property embarked in the mining interests of the Western counties, and are aware that the annual value of the mineral produce of Cornwall and Devon alone has recently amounted to 1,340,000*l*.

In the chapter on Economic Geology, which forms part of the Memoir connected with his Map of Cornwall and Devon, Mr. De la Beche has placed, in a more prominent light than has ever yet appeared, the bearing of geological researches and mineral statistics upon political æconomy; and proves, by tabular documents, the important fact, that the average value of the annual produce of the mines of the British Islands amounts to the enormous sum of 20,000,000*l**, of which about 8,000,000*l*. arise from iron, and 9,000,000*l* from coal.

* See Geological Report on Devon and Cornwall, p. 624, and note, 1839. In this estimate the value of the copper is taken in the ore, before fusion;

Should this inquiry be extended through the endless departments of art, industry and commerce, which have their origin in the manufactories of metals, and in the power of steam, derived exclusively from the application of coal, the vast national importance of mineral statistics, and of models, maps and sections, on which alone their details can be effectually recorded, must be apparent to every one.

Still more extensive will be the statistical and political importance of the next portion of this great work, now in progress by the same highly accomplished geologist, which is to comprehend the coal and iron districts of Monmouthshire and South Wales.

GEOLOGICAL MAP OF ENGLAND.

You have this day the satisfaction to see suspended in your meeting-room a new edition of Mr. Greenough's Geological Map of England, which has for many years formed the glory of this Society. It is truly gratifying to observe how small a change this new edition exhibits, either in the general dispositions, which it represented nearly a quarter of a century ago, or in the complicated details of the boundaries of the different formations. Some alterations appear in the Greensand series, the Wealden, the Lias, and the New red Sandstone. The principal additions are the introduction of the Silurian divisions made in the slate rocks, by Mr. Murchison, in the border districts of England and Wales; and the new distribution very recently assigned to the slate rocks of Devonshire and Cornwall.

A great improvement also has been made by the substitution of an entirely new Map of Wales and Siluria, founded on the Ordnance surveys of those regions, of which no accurate physical map existed at the time of Mr. Greenough's first publication. Another improvement in the execution consists in the union of linear shadows with the colours representing the superficial extent of the strata. The combined effects of these *elements of expression*, judiciously employed, has been to exhibit, more distinctly, the subdivisions of formations, without destroying the unity of the general mass to which they belong. By the frequent introduction also of conventional signs, and figures of reference, Mr. Greenough has produced a more condensed assemblage of scientific information, of varied kinds, than has been put together in any map of equal extent yet published. Extreme attention has also been paid to the physical features of the country, and in the orographic details more than 500 heights are given. The hydrographic features also are delineated with scrupulous exactness.

GEOLOGICAL MAP OF IRELAND.

The last summer has witnessed the production of Mr. Griffith's large and splendid Geological Map of Ireland, containing the results

that of the iron, lead, zinc, tin and silver, after fusion, in their first marketable condition—as pigs, blocks and ingots. The coal is valued at the pit's mouth.

of nearly thirty years' investigation, by that eminent geologist and civil engineer.

Mr. Griffith had supplied an outline of this map published in the Report of the Railway Commissioners for Ireland, 1838. It is obvious that the information thus conveyed, as to the nature of the materials of which the island is composed, affords the most solid basis for sound calculation as to the future improvement of Ireland by the application of its natural resources.

GEOLOGICAL MAP OF A LARGE PORTION OF EUROPE.

During the last year we have also witnessed the publication of a beautifully coloured general Geological Map of Germany, France, and England, and parts of the adjoining countries, compiled from the larger original maps of Von Buch, Elie de Beaumont, and Greenough, by Professor Von Dechen, in one large sheet, published at Berlin.* This map exhibits the geological details of a larger continuous portion of the surface of the earth than has ever before been put together with so much exactness, and set forth on such eminent authority. It also presents to the statesman and political economist the most important portions of central Europe, under the new aspect of the natural divisions of the mineral formations, of which each country is composed; showing that in every region the nature and disposition of the substrata lie at the foundation, not only of its agricultural productiveness, but also of its capability of supplying the materials, which form the basis of its industry and arts. As an historical document, this map demonstrates the rapid progress of our science, and the state of maturity which it has attained.

Thus far I have occupied your attention with external matters of extraordinary interest in the history of our science, which show that geological knowledge is spreading its salutary influence, more widely and rapidly than heretofore, over the practical business of the country. I now proceed to consider the communications made to the meetings of our Society during the past year.

[To be continued.]

LIX. *Intelligence and Miscellaneous Articles.*

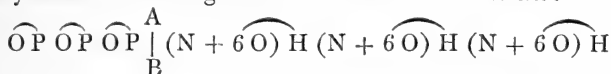
NOTE REFERRED TO IN THE ABSTRACT OF PROFESSOR DANIELL'S PAPER, p. 354.

THE preceding considerations will furnish a satisfactory clue to the apparently anomalous origin of the currents in Becquerel's circuits; when, for example, nitric acid is placed on one side of a diaphragm, and solution of potassa on the other, platinum electrodes being placed in either cell and the circuit completed, oxygen is evolved on the potassa side, and hydrogen shows itself in the acid by its secondary action. "Nitrate of potassa is of course formed at the junction of the acid and alkali. Now let us recollect what nitrate of potassa is in its electrical relations: it is an *oxinitrion of potassium* ($N + 6 O$) + P. Aqueo-nitric acid is also an *oxinitrion of*

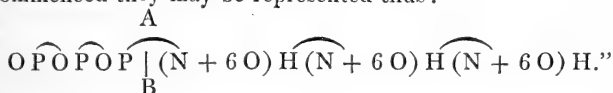
* Schropp and Company, 1839.

hydrogen ($N + 6O$) + H; and potassa is oxide of potassium ($P + O$). In their local action upon each other, the acid and the alkali are both decomposed; the oxinitrion of the former combines with the metal of the latter, and water is formed by the union of the hydrogen and oxygen. This water there is no difficulty in regarding as a separate and secondary product, inasmuch as the salt is incapable of combining chemically with it.

"When a circuit, however, is formed of proper conductors, the compositions and recompositions take place through a series of connected particles, as in the manner of all other electrolytic conduction: and the oxygen and hydrogen, instead of combining together, as in the local action, are respectively evolved at the zincode and platinode. The following diagrams may perhaps assist in explaining my notion of the origin and connexion of the current:—



"Let $\text{O} \text{P}$ and $(\text{N} + 6\text{O}) \text{H}$ represent the two electrolytes on the opposite sides of the diaphragm $\text{A} \text{B}$ before the action; after action has commenced they may be represented thus:—



NEW COMPOUND OF PLATINA.

Messrs. Rogers and Boyé have stated to the American Philosophical Society the existence of a new compound of platina. It is prepared by evaporating a solution of platina in aqua regia to dryness, and adding in small portions at a time a great excess of aqua regia. The compound may be thus readily obtained by filtration, and pressing the powder between folds of blotting-paper. If the concentration of the liquid be carried too far, it is necessary to add just as much water as is sufficient to keep the mass in a semifluid state, and to prevent the precipitation of the deutochloride of platina.

The salt is perfectly well characterized, and is composed of deutochloride of platina and nitric oxide. It is of a gamboge yellow colour, and crystallizes distinctly, though on account of the smallness of the crystals, their form has not been yet determined; it is very deliquescent, and absorbs atmospheric moisture at common temperatures with great avidity. It is rapidly decomposed by the mere addition of water, which causes a brisk effervescence of nitric oxide, and deutochloride of platina remains in solution.

When this compound is heated to 212° , it does not yield its combined water. In order to determine the quantity of platina and chlorine, the salt was fused with carbonate of potash, and the platina thus obtained was weighed, the chlorine was then precipitated by a solution of nitrate of silver. The quantity of nitric oxide was

determined by introducing a portion of the salt into a graduated tube, inverted over mercury, and by decomposing it with water.

The mean of experiments performed in different modes gave as the composition of this substance,—

Chlorine	43·89
Nitric oxide	4·98
Platina	41·26
Water and loss	9·87

100.

BLUE OXIDE OF TITANIUM IN SCORIÆ.

In analysing the blue scoriæ of different countries, M. Kersten found small quantities of titanitic acid, and that these scoriæ possessed a blue colour similar to that of the blue oxide prepared in the dry way; he presumed, therefore, that this colour, instead of being derived from the protoxides of iron and manganese, which sometimes occur in them, might be owing to titanitic acid [oxide?]. According to the facts above stated, it is very probable that titanitic acid, which occurs very often in the ores of iron, after having been dissolved and scorified during the operations of the blast-furnace, is reduced to the state of oxide by the fused iron, similarly to what occurs in the humid way with solutions, and as has happened in several preceding assays. If this assumption be well founded, it ought to be possible, with the substances usually contained in the scoriæ and with titanitic acid, to reproduce blue glass on the small scale. M. Kersten succeeded, not only in fusing together silica, lime, alumina, titanitic acid and iron, all of them pure, and in producing earthy glasses of a blue colour, resembling the blue scoriæ of iron, but also succeeded in obtaining them with the same earths, titanitic acid and zinc free from iron, or with pure tin.

M. Kersten, therefore, concludes from these researches, that the blue colouring matter of many iron scoriæ is the blue oxide of titanium. This blue oxide deserves some attention from its application to the arts, and the author endeavoured to procure blue enamels upon porcelain by using it; and though they were not so fine as those of cobalt, they most nearly approach them.—*L'Institut*, No. 353.

ON THE PROTEIN OF THE CRYSTALLINE HUMOUR.

This substance was discovered by Berzelius, and according to him it constitutes 35·9 per cent. of the crystalline humour, and does not become, like albumen, a coherent mass by coagulation, but a granular one. In other respects its properties are similar.

This new product may be obtained as follows:—

The crystalline humour is to be carefully separated from fifty eyes, they are to be washed, the cells torn, water is to be added and filtered. The liquor heated in a water-bath soon coagulates and deposits clots. After drying, the pounded mass was subjected to the action of alcohol and water, and boiled; after this it was dried at 266° Fahrenheit.

Thus obtained this substance is white, and possesses all the properties of albumen, but is not easy to pulverise it. When it is put into a solution of potash in a silver vessel, the silver is blackened, which shows that it contains free sulphur: it contains no free phosphorus.

The composition of the crystalline humour [protein?] is stated to be

Hydrogen....	6.94	or	62 atoms.
Carbon.....	55.89	—	40 —
Oxygen	21.16	—	12 —
Azote	16.54	—	10 —

The quantity of sulphur is in the proportion of one atom to fifteen atoms of protein.

The entire crystalline humour dissolved in nitric acid, mixed with a solution of nitrate of iron and precipitated by ammonia, furnishes the same quantity of phosphate of lime as a hydrochloric solution of the same substance. The phosphoric acid is derived from the phosphate of lime; the substance therefore contains no free phosphorus.

If the crystalline humour in a very dry state be put into sulphuric acid, it swells, and is converted into a transparent gelatinous mass, like the protein of caseum, fibrin, &c. By the addition of water it contracts and yields a hard powder.

The author concludes that the principle of the crystalline humour is protein, for it possesses the same composition and atomic weight as the pure protein of albumen, fibrin, caseum, &c.—*Journal de Chim. Médicale*, Août, 1840.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1840.

Chiswick.—Sept. 1, 2. Fine. 3. Rain. 4. Cloudy: rain. 5, 6. Fine. 7, 8. Very fine. 9. Hazy. 10—13. Very fine. 14. Hazy: heavy rain. 15. Cloudy: rain at night. 16. Rain, with brisk S.W. wind: barometer exceedingly low. 17. Very fine: frosty at night. 18. Frosty haze: very fine. 19. Cloudy and cool. 20. Fine. 21. Fine: rain. 22. Heavy rain. 23. Rain: clear and fine at night. 24. Heavy showers. 25. Cold and wet. 26. Overcast: rain. 27. Cloudy and fine. 28. Heavy rain. 29, 30. Clear and fine.

Boston.—Sept. 1. Cloudy. 2. Fine. 3. Rain: rain early A.M. 4—6. Fine. 7. Cloudy. 8. Fine. 9. Cloudy: rain early A.M. 10. Fine: rain early A.M. 11, 12. Fine. 13. Fine: rain P.M. 14. Cloudy. 15. Fine. 16. Fine: rain early A.M.: rain P.M. 17. Cloudy: rain early A.M. 18. Fine: rain P.M. 19, 20. Cloudy. 21. Cloudy: rain P.M. 22. Stormy and rain: rain A.M. 23. Rain: rain early A.M. 24. Fine: rain early A.M. 25. Rain: rain early A.M.: rain A.M. 26. Fine: rain P.M. 27. Fine. 28. Cloudy. 29. Fine: rain P.M. 30. Fine.

Applegarth Manse, Dumfries-shire.—Sept. 1. Fine harvest day: air electric. 2. Rain from midday. 3, 4. Showery. 5. Fine and clear. 6. Fine but cloudy. 7. Fine: a few drops of rain. 8. Cloudy A.M.: rain P.M. 9. Wet: cleared up: wet again. 10, 11. Occasional heavy showers. 12. Moist, but moderate. 13. The same: one shower. 14. Fine and clear. 15. Cold and showery. 16. Rain A.M. 17, 18. Very fine. 19. Fine A.M.: moist P.M. 20. Fine A.M. 21. Fine A.M.: showery. 22. Fine and dry: thunder A.M. 23. Rain. 24. Fine and fair. 25—27. Very wet. 28, 29. Moist. 30. Showery.

Sun shone out 28 days. Rain fell 21 days. Thunder 1 day.

Days of Month, 1840. Sept.	Barometer.			Thermometer.				Wind.			Rain.			Dew point. Lon- don: Roy. Soc. 9 a.m.					
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.		Chiswick.	Bost. 8 p.m.	Dum- fries- shire.	Chiswick.	Boston.		Dumfries- shire.				
		Max.	Min.		Fahr. 9 a.m.	Self-register. Max. Min.	Max.	Min.								Max.	Min.		
1.	30.034	29.965	29.713	29.48	30.04	29.77	65.9	72.9	54.3	80	58	62	64	47½	E.	E.	63
2.	29.756	29.689	29.559	29.07	29.48	29.35	69.8	76.2	63.9	78	53	69	65½	51	s. calm	se, sw	66
3.	29.664	29.727	29.569	29.00	29.42	29.49	60.3	75.7	56.3	66	43	56	59	45½	w. calm	sw.	61
4.	29.826	29.774	29.735	29.26	29.40	29.58	61.8	64.2	52.6	62	46	56	58	47	w. s.	w. s.	58
5.	30.012	30.063	29.927	29.44	29.75	29.95	60.0	64.8	51.2	68	40	67.5	58½	43½	w. calm	s.	57
6.	30.276	30.196	30.112	29.50	30.03	29.90	59.8	67.2	50.8	74	53	62	59	44	s. calm	s.	55
7.	30.132	30.066	30.005	29.50	29.80	29.91	62.6	69.8	57.3	72	44	62	60	54	sw. calm	sw.	58
8.	30.180	30.105	30.073	29.58	29.88	29.78	58.4	66.7	52.3	71	51	58	57½	44	sw. wsw.	sw.	56
9.	30.044	29.986	29.880	29.37	29.58	29.68	62.3	66.7	56.7	68	59	60	64	53	s. ssw.	sw.	57
10.	30.014	29.962	29.917	29.35	29.69	29.70	61.3	68.0	60.7	72	45	59	64	54	wnw.	w. w.	59
○ 11.	29.994	29.933	29.901	29.30	29.66	29.63	60.0	66.8	51.9	69	40	57.5	54½	45	sw.	w. wsw.	55
12.	30.024	29.968	29.894	29.38	29.64	29.74	56.8	65.3	47.2	66	35	54	56	45½	w. w.	w. nw.	52
13.	29.900	29.856	29.643	29.27	29.67	29.52	53.8	62.2	45.2	65	34	54	58	43½	w. w.	w. w.	48
14.	29.474	29.455	29.197	28.95	29.31	29.31	54.3	60.7	46.0	54	43	48.5	53	40½	e. calm	e by s.	49
15.	29.314	29.290	29.100	28.75	29.27	29.04	51.2	59.4	45.9	59	41	50	55	42½	w. w.	n. by e.	50
16.	28.892	29.079	28.744	28.31	28.76	29.02	55.7	60.8	46.9	57	43	50	51½	44	sw.	w. n. e.	49
17.	29.550	29.718	28.470	28.88	29.32	29.68	55.3	60.0	45.7	62	49	50.5	53½	44½	sw var.	w. n. e.	48
18.	29.896	29.875	29.856	29.36	29.90	30.01	51.3	59.3	41.7	62	48	53	55½	40	w. n. e.	n. n. e.	45
19.	29.880	29.930	29.857	29.37	30.00	29.93	53.3	59.7	49.3	57	38	48.5	56	37½	nnw.	n. n. e.	49
20.	30.036	30.002	29.995	29.50	29.87	29.80	51.3	56.6	42.4	62	32	48	54½	38	wnw.	w. calm	44
21.	30.026	29.988	29.735	29.40	29.63	29.48	54.5	57.8	45.0	63	52	51.5	56	48	s. s.	w. sw.	45
22.	29.500	29.497	29.482	28.87	29.20	29.15	55.2	52.4	53	40	51	56	42	48	s. s.	sw.	50
23.	29.530	29.470	29.301	28.85	29.	29.42	51.4	56.7	44.6	60	39	48	53	42	wsww.	s. calm	45
24.	29.530	29.571	29.455	29.06	29.53	29.67	53.2	58.8	48.0	60	48	54	55	42	e. calm	s. calm	48
● 25.	29.796	29.984	29.702	29.30	29.83	29.94	50.2	59.3	50.0	57	35	52	57	35	nnw.	n. calm	49
26.	30.062	30.026	29.874	29.54	29.74	29.75	50.7	56.5	45.4	63	42	47	54½	41½	sw.	sw. calm	46
27.	30.014	29.967	29.899	29.36	29.62	29.60	55.4	60.6	49.0	65	49	54	53½	45	s. s.	sw. calm	46
28.	29.790	29.757	29.514	29.10	29.25	29.09	56.3	63.8	51.3	59	48	53	57	50	se.	sw. nw.	52
29.	29.624	29.677	29.583	28.94	29.15	29.40	55.5	60.3	50.2	60	42	52	50½	46	s. s.	sw. w.	53
30.	29.998	29.947	29.934	29.30	29.65	29.58	52.8	59.0	46.3	60	51	51	53	43	w. calm	sw.	49
Mean.	29.821	29.817	29.686	29.21	29.575	29.589	67.3	63.3	50.0	53.93	44.03	54.3	56.6	44.4			Sum. 1.84	2.45 1.90	3.54 52

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

DECEMBER 1840.

LX. *On the Minerals found in the Neighbourhood of Glasgow.*
By THOMAS THOMSON, M.D., F.R.S. &c., *Regius Professor*
of Chemistry in the University of Glasgow.*

PERHAPS there is no part of Great Britain, not even excepting Cornwall, or the mining counties of the north of England, so rich in mineral species as the neighbourhood of Glasgow, if we include under that appellation Lead Hills and Wanlock Head, and the ranges of mountains on both sides of the Clyde, constituting the Kilpatrick hills, and the ranges of hills behind Greenock and Port Glasgow, extending as far as Kilmacolm.

The mines at Lead Hills began to be wrought during the reign of James IV., under the name of gold-mines, and it is said by Boethius that he extracted from them a considerable treasure. The jaspers, rubies and diamonds which Boethius describes in glowing terms, were doubtless the different species of lead ore which still exist in these mines, namely, the sulphate, carbonate and phosphate of lead, remarkable at once for the beauty of their colours, their figure and their lustre.

In the time of James V. Lead Hills was a lead mine, as it is at present; and it is particularly described by Agricola in his celebrated work, *De re metallica*. Now Agricola died in the year 1555.

Becher, who first attempted to establish a theory in chemistry, visited the Lead Hills mines in the year 1682, and ever since that period they have been wrought to a considerable extent.

The principal ore at Lead Hills is galena or sulphuret of

* Read at the British Association at Glasgow, and now communicated by the Author.

lead, which occurs in the mine in all the different varieties which that prolific mineral is known to assume. The gangue of the vein which accompanies the ore is most commonly sulphate of barytes, which occurs in abundance, and in a state of great purity. Calcareous spar also is common, and arragonite is met with in very beautiful crystals.

Besides galena, no fewer than nine species of lead ore occur in Lead Hills, some of which have not hitherto been observed anywhere else.

Of these nine species I may take a short review.

1. *Sulphate of lead*.—Beautiful specimens of this species of lead ore occur at Wanlock, crystallized, and perfectly white. The crystal is a right rhombic prism, and the lustre is adamantine, but I think less so than that of carbonate of lead. Magellan, in his English edition of Cronstedt's *Mineralogy*, published in 1788, says, that this species was discovered by Monnet, but I do not find any allusion to sulphate of lead in Monnet's *Mineralogie*, published in 1779; so that the discovery of its composition must have been later than that period. Klaproth first subjected this mineral to analysis in 1802. The specimens which he examined were from Anglesey and from Wanlock Head. I analysed a specimen from Wanlock Head, and found it contained no other foreign matter than a trace of water. It constitutes one of the rarest of the ores of lead.

2. *Carbonate of lead*.—This species is much more abundant than the last. It occurs pretty frequently in crystals, constituting right rhombic prisms, considerably more oblique than those of the sulphates: the composition of carbonate of lead ore was known to Monnet in 1779. It has the diamond lustre in great perfection. This species, when made artificially, is a well-known paint called *white-lead*. It was first analysed by Klaproth in 1802. The specimen which he examined was from Lead Hills; I analysed a very fine crystal from the same locality, and found it a pure carbonate of lead, with the exception of a mere trace of water.

3. *Cupreo-sulphate of lead*.—This species of lead ore was first noticed by Mr. Sowerby, and so far as I know, it has been hitherto met with only at Lead Hills. It has the colour of the finest specimens of blue carbonate of copper. Its shape approaches very nearly to that of sulphate of lead.

Mr. Brooke analysed a few grains of it, and found it composed of one atom sulphate of lead, one atom oxide of copper, and one atom of water. By the kindness of Mr. Brown, I got a specimen sufficiently large to enable me to subject it to analysis; the specific gravity was 5.2137. I obtained

Sulphate of lead ...	74·8, or 1 atom.
Oxide of copper ...	19·7, or 1 atom.
Water.....	5·5, or $1\frac{1}{4}$ atom.

100·0

It is therefore a compound of one atom of sulphate of lead and one atom of hydrate of copper. The blue colour shows that the oxide of copper which it contains is combined with water.

4. *Sulphato-carbonate of lead.*—This species has been hitherto observed only at Lead Hills, at least so far as I know; the colour is yellowish or greenish white; it is usually crystallized in oblique four-sided prisms. Mr. Brooke analysed it and described it in 1820. By the kindness of Mr. Brown, I got a sufficient quantity of it to subject it to analysis. I found the specific gravity to be 6·3197, which is rather less than that given by Mr. Brooke. My analysis agrees very nearly indeed with that of Brooke. I obtained

Carbonate of lead	46·04
Sulphate of lead...	53·96

100·00

showing that it is a compound of one atom of carbonate and one atom of sulphate of lead combined together.

5. *Sulphato-tricarbonate of lead.*—This species is also peculiar to Lead Hills. It was first noticed by Bournon. Mr. Brooke described and analysed it in 1820. By the kindness of Mr. Brown, I was enabled to subject it to analysis a few months ago. The lustre is splendent, and the colour yellowish-white. It occurs crystallized both in rhomboids and prisms. I found its specific gravity exactly 6·000. Its constituents were

Carbonate of lead	72·57, or 3 atoms.
Sulphate of lead	27·43, or 1 atom.

100·00

This almost coincides with the previous analyses of Brooke and of Stromeyer, who also subjected this mineral to analysis.

6. *Phosphate of lead.*—This species occurs in Lead Hills and Wanlock Head in considerable quantity, and exhibits a variety of colours, chiefly green, yellow and brown. It is frequently crystallized in six-sided prisms, but more commonly in these mines in cauliflower-like vegetations.

It is curious that phosphate of lead has never yet been found native except mixed with chloride of lead. The proportions vary somewhat; but the average may be stated one

atom of chloride of lead and eight atoms subsesquiphosphate of lead. There is usually a small quantity of oxide of iron present, to which in all probability the colour of the mineral is owing. I examined five specimens of this mineral from Lead Hills; the average quantity of chloride of lead in them was $10\frac{1}{4}$ per cent.; the average quantity of protoxide of iron was $1\frac{1}{3}$ per cent.; the rest was a compound of $1\frac{1}{2}$ atom of oxide of lead and one atom of phosphoric acid.

The colour of these specimens was grass-green, olive-green, yellow and brown. The specific gravity varied from 6·70016 to 6·5781. None of them contained any phosphate of lime. This was the case also with the specimens analysed by Wöhler and Berthier, and with those that Klaproth had analysed. But some years ago Mr. Charles Kersten of Freyberg published an analysis of seven specimens of phosphate of lead from various localities, five of which contained phosphate of lime, varying in the different specimens from 12 per cent. to 0·59 per cent. The specific gravity of these specimens, which contain much phosphate of lime, is lower than of the others. That which contains 12 per cent. has a specific gravity of only 5·092.

Some months ago I got two specimens of phosphate of lead from Mr. Brown, locality Lead Hills, still lighter than any I had before seen. The first specimen was of a light greenish-yellow colour, and a slaty texture, and had a specific gravity of only 5·366. It contained no less than 15 per cent. of phosphate of lime.

The other specimen was dark-green, and constituted a botryoidal concretion upon the surface of the first kind. Its specific gravity was 5·970, and it contained rather more than nine per cent. of phosphate of lime.

From the great difference of the quantity of phosphate of lime in different specimens, it would seem rather to be mechanically mixed with than chemically combined with the phosphate of lead.

7. *Cupreo-sulphato-carbonate of lead.*—This is the scarcest of all the ores of lead which occur at Lead Hills. It has a fine green colour, precisely like that of malachite, and occurs most frequently in mineral crystals, having for their primary form, according to Brooke, a right rhombic prism with angles of 95° and 85° . It was first noticed by Mr. Sowerby under the name of green carbonate of copper. It was first described and analysed by Mr. Brooke in 1820.

I could only employ for analysis 3·29 grains of this mineral; but as the analysis is simple, and I employed every precaution I could think of to avoid error, I have reason to believe that

the result approaches pretty near the truth. The specific gravity by my trials was 5·000, but the specimen was not absolutely pure.

From 3·29 grains, I obtained

Sulphate of lead . . .	1·74
Carbonate of lead . . .	1·05
Oxide of copper . . .	0·44—3·23.

The 0·06 gr. wanting to make up the original weight was foreign matter and water.

According to this analysis 100 parts of the ore would have given

		Atoms.	
Sulphate of lead . . .	52·88	1·46	$1\frac{1}{2}$
Carbonate of lead . .	31·91	1	1
Oxide of copper . .	13·37	1·4	$1\frac{1}{2}$
Impurity and water	1·84		
	100		

Mr. Brooke supposes the copper to exist in this mineral in the state of carbonate, but the result of the analysis just stated, is quite inconsistent with that opinion. It would, however, be desirable, in order to be quite certain, to repeat the analysis on a larger quantity of the mineral than I could procure.

8. *Chromo-phosphate of lead*.—This beautiful mineral exists in the Wanlock Head mine in considerable quantity. It has a beautiful orange colour, without any of the red tinge which characterizes chromate of lead. It is often in cauliflower concretions; but it occurs also crystallized in six-sided prisms, constituting the common form which characterizes phosphate of lead. I ascertained many years ago that this beautiful orange colour which distinguishes this ore was owing to the presence of about two per cent. of chromate of lead; the rest was a mixture of subsesquiphosphate of lead and chloride of lead in nearly the same proportions as in the green varieties.

9. *Vanadate of lead*.—This very rare ore has hitherto been found only in Mexico and at Lead Hills. It was analysed in 1804 by Del Rio, who announced the existence in it of a new metal. This was contradicted by Collet Descotils, who affirmed that what Del Rio took for a new metal was chromium; and certainly the resemblance between chromium and vanadium is considerable.

The specimens in my possession I got several years ago from Mr. Doran, an Irish mineral dealer, who told me that he picked them up in an old neglected lead mine in the county of Wicklow, in Ireland. But there is reason to suspect that he was mistaken.

The colour is light brownish yellow, and, like phosphate of lead, it is crystallized in six-sided prisms.

My specimen was analysed a good many years ago by my nephew, Dr. R. D. Thomson, who found it a compound of 1 atom of chloride of lead, and nearly 9 atoms of sesquioxide of lead*.

Besides these ten species of lead ore which occur at Lead Hills or Wanlock Head, there are found also native copper and malachite, or hydrous dicarbonate of copper; a beautiful green-coloured mineral often crystallized, and when large specimens occur, as the well known ores from Siberia, they are often cut into ornamental tables, &c.

The Lead Hills malachite is rather a dark green, but the specimen of it subjected to analysis was very nearly pure.

Lead Hills also contains specimens of blende or sulphuret of zinc. By far the finest specimens of hydrous silicate of zinc which I have seen, are from Lead Hills; they constitute beautiful white crusts or plates. Its specific gravity is 3.1645, and its constituents, as determined by analysis, are

Silica	23.2	1
Oxide of zinc	66.8	1.12
Water	10.8	0.82

100.8

or it is a simple silicate of zinc with rather less than an atom of water. It is curious that in all the analyses of this mineral hitherto made the water never amounts to an atom. In that made by Mr. Smithson the water was only $4\frac{1}{2}$ per cent. By Berzelius's analysis in 1819, it amounted to $7\frac{1}{2}$ per cent., and in one analysis of a specimen from Lead Hills it was 10.8 per cent., or almost 11 per cent. The probability is that it undergoes a kind of efflorescence by keeping, and that originally the water in the ore was exactly an atom. If that supposition be correct, the water originally must be $13\frac{1}{2}$ per cent.

The Kilpatrick hills, which bound the valley of the Clyde from the Stockey muir to Dumbarton, and also the corresponding but lower range on the south side of the valley are composed of various trap rocks, among which amygdaloid is pretty common. Now amygdaloid is a rock full of almond-shaped cavities of various sizes, usually filled up by crystallized minerals, many of which, though not the whole, belong to the beautiful tribe of zeolites. We may, therefore, divide the minerals found in these mountains into two sets: 1. The *zeolites*, so called because they froth before the blow-pipe, and they owe this frothing property to the great quantity of

* Records of General Science, vol. i. p. 34.

water which they contain, and which is easily driven off by heat. 2. Minerals nearly destitute of water, which in general, though not in all cases, exist in greater quantities than the zeolites, and may be often considered as constituting an integral portion of the substance of the mountain in which they occur.

The zeolites, including under the term one or two minerals, which agree with them in containing water and in the way in which they occur, amount to about fifteen species, and there are several which are peculiar to these mountains, or at least have not hitherto been met with anywhere else. I shall take a short view of them.

1. *Stellite*.—This species has hitherto been observed only in the rift of a greenstone rock situated on the banks of the Great Canal, a little to the east of Kilsyth. The rock had been blasted, and a good deal of it brought to the neighbourhood to mend the horse-path of the canal. My son accidentally observed the *stellite* as we were walking on the banks of the canal, and drew my attention to it.

It is snow-white, and always in crystals, which issue like rays from several centres. Each circle of crystals is firmly attached to the rock. The shape of the crystal cannot easily be determined, from its small size and irregularity of form. It is rather harder than calcareous spar, and has a specific gravity of 2.612. This exceeds that of zeolite in general, showing that it contains less water than most others. But it agrees with the zeolites in melting with effervescence before the blow-pipe into an enamel. It is composed of $4 \text{ Cal S}^2 + \text{Mg S}^2 + \text{Al S} + 2\frac{1}{2} \text{ Aq}$, according to the analysis of myself and Dr. R. D. Thomson.

2. *Thomsonite*.—This mineral, together with the three that are to follow it, were confounded together by Cronstedt under the name of *zeolite*, in his paper on the subject published in the Memoirs of the Stockholm Academy for 1756. In his Mineralogy he attempted to subdivide the zeolite into species, but his descriptions are too imperfect to enable us to recognize the minerals to which he alludes. Werner afterwards divided the zeolites into needlestone, radiated zeolite and foliated zeolite. Haüy afterwards attempted a more exact arrangement, and divided the zeolites into two species, which he distinguished by the names of *mesotype* and *stilbite*. In 1817, Fuchs and Gehlen made an accurate chemical analysis of a number of zeolites, and showed that the mesotype of Haüy contains three distinct species, which they distinguished by the names of *natrolite*, *mesolite* and *scolezite*. In 1820, Mr. Brooke, without being aware of what had been done by Fuchs and

Gehlen, showed that the mesotype of Häuy ought to be divided into three species, which he distinguished by the names of mesotype, needlestone, and Thomsonite. The first two of these constitute the natrolite and scolezite of Fuchs and Gehlen; but the third is a new species, which Mr. Brooke first described. He showed that these minerals differ in their crystalline shape and in their specific gravity; Thomsonite being the heaviest, and natrolite or mesotype the lightest.

Thomsonite is a beautiful mineral, and is rather abundant both in the Kilpatrick hills, and those on the south side of the Clyde. When pure it is snow-white; the crystal is a right rectangular prism. The lustre is vitreous, and the specific gravity is about 2.37, according to Brooke; though I have found it as low as 2.29. Before the blow-pipe it swells. It contains about 13 per cent. of water, and is composed of $3 \text{ Al S} + \text{Cal S} + 2\frac{1}{2} \text{ Aq}$.

There is a curious variety which occurs at Ballimony, in the north of Ireland; its colour is brown, and it is not crystallized. Its specific gravity is only 2.29; but its chemical analysis shows the same constitution as the Thomsonite of Kilpatrick*.

There is another variety which occurs at Port Rush, which deserves attention. It constitutes small spheres about the size of a pea in an amygdaloidal rock; these spherules are composed of needles radiating from the centre to the circumference. Colour white, with a slight tinge of yellow; translucent. Lustre vitreous. Specific gravity 2.366, and it is rather softer than common Thomsonite. It froths before the blow-pipe, like the other zeolites. Its composition is the same as that of Thomsonite; but its proportion of alumina is rather less, and it contains $6\frac{1}{3}$ per cent. of soda, and the proportion of water is less. If we reckon

Thomsonite $3 \text{ Cal S} + 13 \text{ Al S} + 10 \text{ Aq}$, the Port Rush mineral will be $3 \text{ Cal S} + 12 \text{ Al S} + \text{N S} + 8 \text{ Aq}$.

My son, to whom I was indebted for the chemical analysis and description of this mineral, distinguished it by the name of Scoulerite, in honour of Dr. Scouler, to whose zeal and abilities the mineralogy of Ireland lies under so many obligations.

3. *Natrolite*.—This mineral constitutes the mesotype of Brooke. The name natrolite was first applied to a yellow mammillary variety from Högan, near Lake Constance, in which he discovered no less than $6\frac{1}{2}$ per cent. of soda. When pure it is quite white, and the crystal is a right rhombic prism, deviating by only $1^\circ 10'$ from a rectangular prism. The

* Analysed by Dr. R. D. Thomson.

prism is commonly terminated by eight faces, constituting two four-sided prisms of different inclinations.

It contains about $16\frac{1}{2}$ per cent. of soda, and $9\frac{1}{2}$ per cent. of water. The other constituents are silica and alumina.

Its constitution is $3 \text{ Al S} + \text{N S}^3 + 2 \text{ Aq}$.

Mesolite.—Natrolite in a pure state, that is perfectly destitute of lime, is uncommon in this neighbourhood. Almost always we find a mixture of soda and lime constituting the *mesolite* of Fuchs and Gehlen; they gave it that name because they considered it as intermediate between natrolite and scolezite, or composed of an integrant particle of natrolite united to an integrant particle of scolezite.

Mesolite is abundant both in this locality and in various other parts of Scotland, where trap rocks occur; and we find it in two different states.

(1.) In four-sided prisms generally united so firmly to each other, that we cannot easily separate them. Fine specimens of this variety are found at Bishoptown in Renfrewshire, where a hill is perforating for a tunnel, constituting part of the Greenock and Glasgow rail-road, at present forming. This variety approaches very nearly to natrolite; since it contains only $1\frac{1}{2}$ per cent. of lime, while there is no less than $13\frac{1}{3}$ per cent. of soda, and a small portion of potash. If natrolite be



this variety will be



Its specific gravity is 2.1365.

The other variety constitutes veins in the rock. It is much softer, has a foliated texture, and a specific gravity of 2.212.

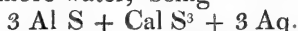
It contains $6\frac{1}{2}$ per cent. of soda and 3 per cent. of lime; so that the atoms of soda are very nearly twice as numerous as those of lime. In other respects its constitution agrees nearly with the other variety.

4. *Scolezite*.—This is the mineral which Werner distinguished by the name of needle zeolite. It occurs often in long needles, as fine as human hair, and quite flexible; sometimes in needles of a longer and larger size, and irregularly interlaced; sometimes the needles are firmly attached to each other, so as to give it a splintery appearance. In this state it is harder than calcareous spar; whereas the hairy variety is quite soft. In some specimens the central portion is hard and splintery, while the outer portion is quite soft and friable. We would naturally suppose that this friability was owing to the loss of a portion of water; but on examining some re-

markable specimens of this kind in my possession, I found that the friable portion contained more water than the hard and splintery.

The crystalline shape of scolezite is the same as that of natrolite, a right rhombic prism. The obtuse angle is $10'$ greater than in natrolite, being $91^{\circ} 20'$, while that of natrolite is $91^{\circ} 10'$.

Scolezite differs from natrolite in lime replacing the soda. It contains also more water, being



It deserves attention, that though in some specimens the soft hairy portion of this mineral appears to be a continuation of the fibres of the splintery central portion, yet the constitution of the two portions is not the same.

I analysed a remarkable specimen of this kind from the Giant's Causeway. The result was as follows:—

Central splinters.		
Silica.....	48.88	46.0
Alumina	26.36	27.6
Lime with trace of ♂	7.64	15.2
Magnesia	2.46	—
Soda... ..	4.20	—
Water	12.32	14.35
	<hr/>	<hr/>
	101.86	103.15

The difference will be best seen by stating the following formulas:—

Central splintery part



Soft outer part



5. *Glottalite*.—The specimen of this mineral in my possession is, I believe, from the hills behind Port Glasgow: at least I so understood Mr. Clachers, from whom I got it. The specimen is unique.

It is white, crystallized apparently in octahedrons, has a vitreous lustre, is translucent, is harder than calcareous spar, and has a specific gravity of 2.181. It contains $21\frac{1}{4}$ per cent. of water. Its constituents are $\text{Cal S} + \text{Al S}^{\frac{1}{2}} + 3 \text{ Aq.}$

6. *Laumonite*.—This mineral was found occasionally in the Kilpatrick hills, but beautiful specimens have been recently obtained at Bishoptown, while digging the tunnel for the rail-road.

Its colour is white, usually with a shade of red. It contains $16\frac{1}{2}$ per cent. of water, which it loses spontaneously

when exposed to the atmosphere, and falls to powder unless protected by a coat of gum.

It is usually crystallized, and its primary form is an oblique rhombic prism. The acute angles of the prism are of $84^{\circ} 30'$, and the base of the prism makes with the lateral faces angles of $114^{\circ} 54'$, and $65^{\circ} 6'$. The specific gravity is 2.361, and it is harder than calcareous spar.

Its constitution is $3 \text{ Al S}^2 + \text{Cal S}^2 + 5 \text{ Aq}$. The only difference which I have found in the Bishoptown laumonite is the presence of 2 per cent. of magnesia in it. Neither Leopold Gmelin, nor Vogel, nor Dufresnoy, who analysed this mineral, take any notice of magnesia as a constituent. I was led to look for it in consequence of the considerable deficiency which occurred in my analysis. Vogel gives $2\frac{1}{2}$ per cent. of carbonic acid. This must be given merely to make up the deficiency in his analysis. It would not be surprising if his specimen, like mine, had contained magnesia.

7. *Chabazite*.—This name was applied by Box d'Antic to our mineral. The Greek term $\chi α β α ζ ι ο ς$ occurs in a poem ascribed to Orpheus, in which twenty kinds of stones are celebrated for their medicinal virtues, but without any description. The last of the twenty is $\chi α β α ζ ι ο ς$. Chabazite occurs pretty frequently in this neighbourhood, particularly about Kilmacolm. It is always in rhomboids, approaching nearly to the cube, the obtuse angles being $94^{\circ} 56'$. It is usually transparent, and has a vitreous lustre. The specific gravity of the rhomboidal crystals is from 2.076 to 2.088. Its constituents are

Silica.....	49.20	24.6
Alumina.....	17.91	7.96
Lime.....	9.64	2.75
Potash.....	1.92	0.32
Water.....	20.41	18.14 ^{11.03}

99.08

$3 \text{ Al S}^2 + \text{Cal S}^2 + 6 \text{ Aq}$.

There is another variety of chabazite which never occurs here, but is pretty common in the north of Ireland. The crystal is never rhomboid, but a very peculiar figure, of which the only way of forming an idea is to examine the model. The primary faces are nearly all concealed; yet it cleaves easily in the direction of the primary faces and yields a rhomboid similar to the primary crystal.

Now this variety differs from the common chabazite of this neighbourhood, both in its specific gravity and its composition. Its specific gravity is 2.472.

And its constituents (the Port Rush variety) are

Silica.....	48.988	24.49	
Alumina	19.774	8.8	... 3
Soda	6.066	1.51	} 1
Lime.....	4.068	1.16	
Protoxide of ♂	0.404		
Water	20.700 6

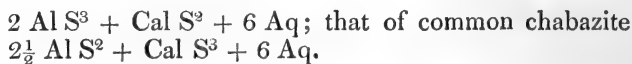
100.000

Here the constitution is the same as in the rhomboidal variety, excepting that a considerable portion of the lime is replaced by soda. In a specimen from Oberstein, analysed by Berzelius, there was no lime at all, but only soda. From this it is obvious that chabazite, like mesotype, is divisible into three species or varieties. The first or rhomboidal chabazite is a hydrous compound of bisilicate of alumina and tersilicate of lime. In the trihedral variety the tersilicate of lime is replaced by tersilicate of soda; and the specimens from Port Rush contain both tersilicate of lime and tersilicate of soda, being analogous to mesolite.

I believe it will be ultimately found that every modification of a crystalline form is occasioned either by a difference in the constitution, or by the action of some peculiar foreign matter during the act of crystallizing.

It may be worth while to mention, that a red-coloured chabazite occurs at Kilmacolm; its characters correspond with those of common chabazite; and its constituents are the same, excepting that it contains 2.75 per cent. of oxide of iron, to which, doubtless, its red colour is owing.

But about two months ago I received from Messrs. Jackson and Alger of New York (well known as the authors of an interesting and valuable paper on the geology of Nova Scotia), a specimen of a mineral which they have distinguished by the name of Acadiolite or yellow chabazite. It is in crystals, having the common form of chabazite, and its other properties (colour excepted) are the same as those of common chabazite. Its specific gravity is 2.0202. But it contains rather more silica and less alumina than common chabazite; and there was found in it 2.4 per cent. of red oxide of iron, to which its colour was doubtless owing. It contains neither potash nor soda, but simply 11.6 per cent. of lime. The formula for its constitution is



The deficiency of alumina in the acadiolite, which amounts

to 5·5 per cent., prevents us from reducing it under the common formula of chabazite. But the formula which it gives is simpler, and may ultimately be found to belong to pure chabazite.

8. *Analcime*.—This mineral, which Werner distinguished by the name of *cubizite*, was first noticed by Dolomieu. It occurs rather abundantly in the Kilpatrick hills. In these hills it is white and translucent; but in the Giant's Causeway in Ireland, transparent crystals of it are found. It is crystallized in cubes, whose angles are usually replaced by three planes. In most crystals these planes have increased so much in size as to obliterate the primary faces. The crystal is then an imperfect sphere bounded by twenty-four equal trapezoidal planes. This figure is well known to mineralogists by the name of the *leucite crystal*.

It is but an imperfect zeolite; yet it melts before the blow-pipe, though without ebullition; and like the other zeolites, is soluble in muriatic acid. Its constituents are silica, alumina, soda and water.

	Atoms.
Silica..... 55·36	27·68
Alumina 23·00	10·22 2·8
Soda 14·19	3·55 1
Water 8·08	7·18 ^{13·77}
	<hr/>
	100·63



The translucent and transparent analcimes have the same composition.

9. *Cluthalite*.—I have given this name to a mineral which occurs in the Kilpatrick hills near Dumbarton, forming large nodules in the amygdaloid of that district.

Its colour is flesh-red; it forms a congeries of imperfect crystals, seemingly rectangular prisms. It is nearly opaque, has a vitreous lustre, is harder than calcareous spar, and has a specific gravity of 2·166.

Its constitution bears a certain analogy to analcime, the alumina being partly replaced by magnesia, and the soda by peroxide of iron.

Silica.....	51·266	...	9·35
Alumina	23·560	}	4
Magnesia	1·233		
Soda	5·130	}	1
Peroxide of iron ...	7·306		
Water	10·558	...	3·43
	<hr/>		
	99·048		



But it contains an additional atom of bisilicate of alumina and of water.

10. *Stilbite* and *Heulandite*.—These two species are found in great abundance in the Kilpatrick hills and at Carberry in the valley which divides the Campsie hills from the Kilpatrick hills. They are always, or almost always, of a deep flesh-red colour, and generally crystallized, though seldom in very regular shapes. These two species were first distinguished from each other by Werner, who gave to stilbite the name of *radiated zeolite*, and to Heulandite that of *foliated zeolite*. Haüy confounded the two under the name of stilbite. Mr. Brooke pointed out the difference between them in 1822; and left Haüy's name *stilbite* to one of the species, while the other was named Heulandite, in honour of Mr. Heuland of London. It is unlucky that the names had not been reversed. The word *stilbite* (from the Greek *στίλβω*, to *shine*) was applied by Haüy, in consequence of the great lustre of the mineral. Now the base of the prism in Heulandite has a much greater lustre than in stilbite, being pearly and splendid. In the specimens from this neighbourhood, the lustre is not so great as in those from the Feroe islands, which are white, and have the pearly lustre in perfection.

The constituents of both these minerals are silica, alumina, lime and water.

Heulandite contains most silica and least water.

Stilbite is $2 \text{ Al S}^3 + \text{Cal S}^3 + 5 \text{ Aq.}$

Heulandite $3\frac{1}{2} \text{ Al S}^3 + \text{Cal S}^3 + 6 \text{ Aq.}$

11. The only other zeolite that occurs in any quantity in the Kilpatrick hills is *harmotome*. It was noticed early by mineralogists under the name of *cross stone*, in consequence of the remarkable way in which the crystals frequently cross each other; that shape is not common in the harmotome of the Kilpatrick hills. It is usually crystallized in right rectangular prisms. But the common modification is a terminal four-sided pyramid, consisting of faces replacing the solid angles of the base of the prism.

The mineral called harmotome ought to be divided into three distinct species, which might be distinguished by the names harmotome, Philipsite, and Morvenite; the first of these only occurs in our neighbourhood; but the first and third are common at Strontian. The second, or Philipsite, has been observed in Sicily, and in other places. The first is a compound of silica, alumina, barytes and water; the second of silica, alumina, lime and water; and in the third the proportions of the constituents differ very much from those of the second species.

Harmotome is $2\frac{1}{2} \text{ Al S}^3 + \text{Br S}^{1\frac{1}{2}} + 4 \text{ Aq.}$

Philipsite is $3 \text{ Al S}^2 + (\frac{2}{3} \text{ Cal} + \frac{1}{3} \text{ K}) \text{ S}^2 + 5 \text{ Aq.}$

Morvenite is $5 \text{ Al S}^4 + \text{Cal S}^4 + 11 \text{ Aq.}$

Morvenite is transparent, while the other two are only translucent.

12. These are all the minerals in the neighbourhood of Glasgow which belong to the tribe of zeolites; they all contain water as an essential constituent; they all froth before the blow-pipe, and they are all soluble in muriatic acid. As they fill cavities in amygdaloid, there is some reason for suspecting that they were deposited in these cavities after the trap rocks had assumed their present form. There is another mineral which occurs in the same situation, which contains water as a constituent, and which is soluble in muriatic acid, I mean the dihydrous peroxide of iron found in a trap rock near Gourcock.

It has a reddish brown, and is composed of very fine needles. But at St. Just in Cornwall, it occurs in crystals, the primary form of which is a right rhombic prism. It has an imperfect metallic lustre, is harder than apatite, and has a specific gravity of 4.375. Its constituents are,

2 atoms peroxide of iron 10

1 atom of water 1.125.

Carbonate of magnesia has also been found recently at Bishoptown; I am not aware of its having been observed in any other part of Great Britain. It abounds in Hindostan, and Hoboken in New Jersey; in both places in serpentine. About two years ago I got a great many very pure specimens from Madras, for which I was indebted to Dr. Cole.

There are twenty other species of minerals which occur in the trap rocks in this neighbourhood; some of them as veins, others making one of the constituents of the rocks. The veins often contain water as a constituent, though not always. But those minerals that enter as constituents into the rocks are destitute of water.

As veins we have sulphate of barytes, calcareous spar, fibrous sulphate of lime, arragonite, Wollastonite and prasolite.

1. Fine specimens of arragonite are found at Lead Hills.

2. The name Wollastonite has been applied by Haüy to bisilicate of lime, the table spar of Werner. But as this new name has not been generally adopted, and as mineralogy lies under the deepest obligations to Dr. Wollaston, I have given his name to a mineral found in considerable quantity in veins in a greenstone rock near Kilsyth, not far from the great canal, and which Lord Greenock has also found near Edinburgh.

It is white, has a fibrous texture, and the fibres are in tufts diverging from a centre. Lustre silky, translucent on the edges, softer than calcareous spar. Specific gravity 2·850.

It is composed of silica, lime and soda, with a little magnesia. If we include the magnesia along with the soda it will be composed of

3 atoms bisilicate of lime.

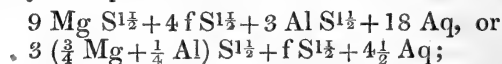
1 atom tersilicate of soda.

$3 \text{ Cal S}^2 + \text{N S}^{3*}$

3. *Prasolite* is a name by which I have distinguished a mineral which occurs in the Kilpatrick hills, constituting a vein about an inch in thickness. It was brought me by one of my students about two years ago, who wanted to know what mineral it was. From its appearance I pronounced it to be common *sulphate of lime*. But happening to examine it a little more closely, I found that I had been mistaken in my opinion, and that it, in fact, constituted a new mineral species, allied to the zeolites; since it contained no less than 18 per cent. of water.

Its colour is leek-green, and it is as soft as Venetian talc, not being capable of scratching selenite. The specific gravity is 2·311. It is composed of fibres very loosely cohering together, since it may be crumbled to powder between the fingers.

Its constituents, besides water, are silica, magnesia, peroxide of iron and alumina, and probably soda. Its constitution may be represented thus:—



Fluor spar is found sparingly in cubic crystals in the trap rocks near Gourrock.

Prehnite is so exceedingly abundant, that at one time a whole cartful of it was brought to Glasgow.

I need not notice augite and amphibole, felspar and albite, which constitute the common constituents of the greenstone rocks so abundant in this part of Scotland. But it is right to notice *labradorite*, which constitutes one of the constituents of a peculiar variety of greenstone which may be seen in a state of perfection in Campsie glen, and which constitutes the principal rock in the hills south of Paisley. In these rocks it is very conspicuous, because they have been altered by the action of the weather, and the labradorite has become white, while the hornblende, the other constituent, retains its dark colour.

* Analysed by myself and Dr. R. D. Thomson. Records of General Science, i. 220.

Labradorite, as the name implies, was first noticed on the coast of Labrador. It is of a dark smoke-gray colour, and in particular positions reflects a variety of colours, blue, green, yellow, red and brown. The crystals approach those of felspar, but differ a little in the measurement of the angles. The constituents are silica, alumina and lime, with a trace of soda. The presence of lime distinguishes it from albite; and the proportion of silica is much smaller.

Felspar is	$4 \text{ Al S}^3 + \text{K S}^3$
Albite.....	$3 \text{ Al S}^3 + \text{N S}^3$
Labradorite	$3 \text{ Al S}^2 + (\frac{2}{3} \text{ Cal} + \frac{1}{3} \text{ N}) \text{ S}.$

I think it unnecessary to mention mica, epidote, steatite, iron pyrites or carbonate of iron, which constitutes so abundant a mineral in this neighbourhood. Nor is it necessary to give an account of the gray ore of manganese which has been met with, though sparingly, in the hills in our neighbourhood. But there are two species of minerals which I believe to be peculiar to the range of hills in our district, and which, therefore, deserve to be described. These are Kilpatrick quartz, and sulphuret of cadmium, or Greenockite, as it has been called.

1. *Kilpatrick quartz*.—This variety of quartz occurs in the amygdaloid of the Kilpatrick hills.

It is white and translucent, and constitutes spheres about the size of a hazel-nut, mixed abundantly with stilbite and calcareous spar. The spheres constitute an aggregation of crystals; the exterior termination of each, examined by the microscope, is a four-sided prism. The hardness is the same as that of common quartz; but the specific gravity is 2.525, while that of common quartz is 2.690. It contains 3 per cent. of water, according to the analysis of Dr. R. D. Thomson, who found also a trace of sulphuric acid; while common quartz is anhydrous.

These differences seem to warrant the propriety of considering it as a peculiar species of quartz. The same mineral exists in the mountains of Nova Scotia, which bear a striking analogy with those in the neighbourhood of Glasgow, both in their constitution and in the minerals which they contain.

2. *Sulphuret of cadmium*.—This mineral occurs along with prehnite in an amygdaloidal rock at Bishoptown in Renfrewshire. It has also been seen on the Cochno burn on the north side of the Clyde. It was mistaken for blende by the mineral dealers in the neighbourhood. Lord Greenock examined it in the summer of 1840, and showed to Professor Jameson that it could not be blende. Mr. Connel subjected

it to analysis, and found it to be pure *sulphuret of cadmium*. His analysis was performed on 3·71 grains.

It is always crystallized in six-sided pyramids. The summit is sometimes replaced by a more acute pyramid: some of the crystals show a six-sided prism.

Translucent to transparent.

Lustre vitreous, or sometimes almost adamantine.

Hardness about 2·75.

Specific gravity 4·534; but the specimen was not quite pure. Mr. Connel states the specific gravity to be 4·842; but the determination was made on 3·68 grains. If the impurity in my specimen was prehnite, the specific gravity of pure sulphuret of cadmium will be 4·546.

On subjecting it to analysis I obtained

Sulphur..... 22·4 or 1·009 atom.

Cadmium ... 77·6 or 1 atom.

100·0

This very nearly agrees with Mr. Connel's analysis, who obtained from 3·71 grains of the mineral,

Sulphur 22·56

Cadmium 77·30—99·86

Professor Jameson has distinguished this mineral by the name of Greenockite, in honour of Lord Greenock, who first discovered it.

LXI. *Deductions from the first Year's Observations at the Magnetic Observatory at Prague: in a Letter from Professor KREIL, Director of the Prague Observatory, to Major EDWARD SABINE, R.A., V.P.R.S.**

Prague, July 21, 1840.

I HAVE seen with great pleasure, by your last letter and its inclosure, that you have made known in English†, my letter to M. Kupffer, containing the results of the Milan Magnetic Observations, and that you have sent it to the newly-established Observatories. I am indeed far from regarding all the deductions from those observations as undoubted facts; I consider them rather as indications afforded by the first made continuous series of observations, and awaiting confirmation or correction from subsequent researches.

Since my arrival at Prague, I have had the advantage of several zealous assistants, and have been able to make the observations with the magnetic instruments at much shorter in-

* A translation communicated by Major Sabine.

† See Lond. and Edinb. Phil. Mag., vol. xvi. p. 241.

tervals than was done at Milan, namely, at every hour from 5 a.m. to 10 p.m. *. This we have done uninterruptedly for a twelvemonth, viz. from July 1839 to June 1840, and I have now the pleasure of laying before you a summary view of the principal results.

1. The variations of the magnetic elements exhibit a dependence on the seasons, for there appear in certain months maxima and minima of which scarcely a trace is discernible in the opposite months. It is therefore desirable to separate the observations of the winter from those of the summer, and to consider each apart. By uniting them we should risk obtaining a daily march which would be wholly imaginary, and would not correspond with the phænomena in *any* month of the year. Thus, for example, the declination in the winter months shows regularly a minimum in the later hours of the evening; in summer the minimum occurs in the morning; the mean of the whole year, therefore, would show two minima, one in the evening, and the other in the morning, whereas the occurrence of two minima on the same day is in every month of the year an exception to the ordinary course. I do not consider the difference in the time of the minima in summer and winter a mere displacement arising from the season; at least it is not so in the case of the inclination, in which a similar difference is observable: but this point must be decided by future observations continued through the hours of the night.

In the winter months, October to March, we observed,
 At 8 a.m. Göttingen mean time, the decl. = $410^{\circ}77'$ $\frac{\text{Diff.}}{16^{\circ}34'}$
 At 1 p.m. ————— the max. = $427^{\circ}11'$ $19^{\circ}27'$
 At 10 p.m. ————— the min. = $407^{\circ}84'$

In the summer months, April to September,
 At 8 a.m. ————— the min. = $398^{\circ}01'$ $29^{\circ}49'$
 At 1 p.m. ————— the max. = $427^{\circ}50'$ $21^{\circ}74'$
 At 10 p.m. ————— = $405^{\circ}74'$

The time of the maximum does not seem to be dependent on the season; the exact time obtained from the hourly observations by interpolation, taking into account the second differences to quarters of an hour, is $0^{\text{h}} 45^{\text{m}}$ in May, September and December; this is the earliest: in February it is at $1^{\text{h}} 45^{\text{m}}$; and the latest occurs in July, viz. at $2^{\text{h}} 0^{\text{m}}$. The monthly means gave the daily difference greatest in August,

* We have now extended our observations so as to include some of the hours of the night.

† As the observations of the absolute declination have not yet been made with the requisite exactness, the variations are here given in divisions of the scale, each division being 27.2261 seconds of arc.

= 36.01; in April 33.01; and least in December, = 8.41, if the observations at 8 a.m. and 1 p.m. are compared, or 13.06 if those at 1 p.m. and 10 p.m. are compared. The secular decrease appears to have been unusually great this year; if the apparatus showed correctly, it amounted from July 1839 to June 1840, to no less than 17.70 divisions of the scale, = 8' 01".9; but as in October, 1839, the thread by which the needle was suspended broke, this result may be inexact. The observations of the following month, however, show a still greater decrease. If we take the mean of all the observations from 5 a.m. to 10 p.m., we obtain the following declinations:

1839. November.	Declination	=	417.80
December.	_____	=	416.88
1840. January.	_____	=	415.80
February.	_____	=	414.74
March.	_____	=	410.35
April.	_____	=	407.50
May.	_____	=	402.87
June.	_____	=	399.30

The declination, therefore, decreased in these eight months 18.50 scale divisions, or 8' 23".7.

2. The horizontal intensity observed with the bifilar had in the winter months its

Minimum = 544.42* at 11 a.m.

Maximum = 564.83 at 10 p.m.

Difference = 20.41

In summer, its

Minimum = 443.63 at 10 a.m.

Maximum = 488.90 at 8 p.m.

Difference..... = 45.27

The minimum was earliest in August, when it took place at 9 a.m., and in June, when it occurred at 9^h 30^m a.m.; it was latest in December, at 1 p.m.; and in January was at 11^h 45^m a.m. The maximum appears to fall in winter, i. e. in December, January and February, during the hours of the night; it was earliest, at 8 p.m. in July, November, April and June. The following are the means of all the observations between 5 a.m. and 10 p.m.

* A scale-division in this apparatus corresponds to 18.5757 seconds of arc, or $\frac{1}{55}$ of the whole horizontal intensity; but it should be remarked, that since the instrument was set up in May, 1839, the magnetism of the bar has not been examined, in order that the series might not be broken.

1839.	July.	Horizontal intensity	=	532·41
	August.	_____	=	465·54
	September.	_____	=	488·85
	October.	_____	=	489·12
	November.	_____	=	559·27
	December.	_____	=	598·58
1840.	January.	_____	=	586·91
	February.	_____	=	552·68
	March.	_____	=	560·01
	April.	_____	=	498·00
	May.	_____	=	437·17
	June.	_____	=	396·01

The numbers here given are not corrected for the influence of temperature, because it appears to affect not only the intensity, but also the direction of the magnetic force, and we must wait for more certain data. The continued and simultaneous observations of the bifilar magnetometer and the inclinorium have shown, indeed, that changes of temperature are always accompanied by changes in the horizontal intensity; but they are also accompanied by corresponding alterations in the dip, which increases as the horizontal intensity decreases, and *vice versâ*.

In some months of the year a decrease takes place in the horizontal intensity between 2 and 5 p.m., which may be ascribed to the maximum of inclination which occurs in those hours; this was previously recognized in the Milan observations.

3. In winter the inclination attains its minimum at 6 a.m., and its maximum at 3 p.m.

Minimum	270·69*
Maximum	272·02
Difference	1·33

In summer the minimum is at 5 a.m., and the maximum at 8 a.m.

Minimum	289·70
Maximum	291·23
Difference	1·53

The several months show at all seasons a small minimum of the inclination about noon, whence we must conclude that

* The value of a scale-division = 28·185 seconds of arc. The apparatus was set up in June, 1839, but was not sufficiently steady to give value to the observations with it until August; it continued, however, subject to a tremulous motion occasioned by passing carriages until February, 1840, when this disadvantage was remedied.

the two maxima, one in the forenoon and the other in the afternoon, always occur, though in the summer months the former is the most conspicuous, and in the winter the latter; the observations which I had previously made at Milan were not sufficiently numerous to manifest the occurrence of this noon-minimum.

Observations made in May and June, 1840, at 2 and 4 a.m., show a maximum and minimum in the hours of the night, which sometimes exceed those above-mentioned.

In May, at 11 p.m. the maximum = 246·43
 at 2 a.m. the minimum..... .. = 244·49

Difference 1·94

In June, at 11 p.m. the maximum = 267·73
 at 4 a.m. the minimum = 266·53

Difference..... 1·20

The circumstance already noticed, that these maxima are in some months almost insensible, makes it difficult to recognize the dependence of the hour of their occurrence on the season of the year.

The following numbers, which are the monthly means of all the observations of the inclination made between 5 a.m. and 10 p.m., will show to what considerable alterations that element is subject in the course of a longer period.

1839. August.	Inclination	= 368·06
September.	————	= 374·42
October.	————	= 366·42
November.	————	= 336·40
December.	————	= 294·18
1840. January.	————	= 252·14
February.	————	= 196·59
March.	————	= 182·91
April.	————	= 197·47
May.	————	= 245·59
June.	————	= 266·70

If we compare these numbers with the means of the horizontal intensity, we shall not, it is true, see any perfectly parallel march, which indeed we ought not to expect, as the horizontal component depends on the total intensity as well as on the dip; but it is sufficiently clear that there is a general accordance, the horizontal intensity increasing with the decrease of dip, and *vice versâ*: therefore the changes of inclination indicated by the instrument are not to be ascribed to mere alterations of the centre of gravity in reference to the point of suspension, but are, in part at least,

due to the altered direction of the force itself. It would seem then that the dip is subject to much greater alterations than has been hitherto recognized; and this is quite conceivable, if we suppose temperature to be one of the chief causes of the variation of the magnetic elements; for if the *daily* progress of the temperature from east to west produces the large *diurnal* variation in the *declination*, its *annual* march from south to north, and *vice versâ*, ought in like manner to occasion an *annual* variation in the *inclination*.

4. The times of oscillation of the dipping needle showed during the winter,—

	s.	Diff.
At 8 a.m. a maximum	= 12·86978	
At 10 a.m. a minimum	= 12·85321	0·01657
At 2 p.m. a second maximum	= 12·86923	0·01602
At 8 p.m. a second minimum	= 12·84823	0·02100

In summer,

At midnight, a maximum.		
At 6 p.m. a minimum	= 12·55078	
At 2 p.m. a second maximum	= 12·57437	0·02359
At 9 p.m. a second minimum	= 12·56605	0·00832

The midnight maximum was shown by the night observations of May and June.

The dependence of the hours of maximum and minimum on the season could not be recognized with certainty from the observations of each month separately considered; it appeared, however, as if those of the forenoon observed in winter approached progressively nearer to noon.

The following are the monthly means of all the observations made from 5 a.m. to 10 p.m.

		s.
1839. August.	Time of vibr.	= 12·02318
September.	————	= 11·86037
October.	————	= 11·78188
November.	————	= 11·90803
December.	————	= 12·69148
1840. January.	————	= 13·36133
February.	————	= 13·40867
March.	————	= 14·00672
April.	————	= 13·76645
May.	————	= 12·77688
June.	————	= 12·39025

From these numbers, which are not corrected for the influence of temperature, or for any decrease in the magnetism of the bar, we cannot trace a connexion with the temperature, such as has been usually supposed to exist, viz. a de-

creased force in increased temperature, and *vice versâ*; but there is a correspondence between these numbers and the monthly means of the inclination; the inclination having decreased and the times of oscillation increased from October to March, whilst subsequently to March the inclination increased, and the times of oscillation decreased. Such observations, continued with instruments somewhat differently constructed, but having the same purpose in view, will soon show what part of this apparent connexion in the variations of the intensity and inclination is due to the instrument, and what to the phænomena themselves.

5. The influence of the moon on the magnetic condition of the earth was examined in the same manner as was done in the case of the Milan observations. Having corrected approximately the observed horizontal intensities for temperature and the loss of magnetism sustained by the bar, there appeared a confirmation of the results previously obtained, viz. that the magnetism of the earth is stronger at the time of the new moon than when the moon is full. The mean of the whole body of the observations showed an intensity

During the last quarter = 549·99

At the new moon = 548·79

During the first quarter = 542·62

At the full moon = 541·11

6. The difficulty referred to in par. 2. of freeing the observations of the horizontal intensity from the influence of temperature, and the uncertainty of the correction for the decrease of magnetism in the bar, induced me to adopt a mode of arranging the observations in reference to the inquiry relatively to the moon, by which the errors arising from both these causes might be avoided. If the moon has an influence on the magnetic condition of the earth, it must produce a daily variation, masked by the greater effect produced by the sun, but recognisable when the latter is eliminated. I constructed, therefore, tables, having for their argument the day of the month, and for the titles of the several columns the different distances of the moon from the magnetic meridian, i. e. her magnetic horary angles. To simplify the calculation, I assumed that the moon passed the magnetic meridian an hour earlier than the true meridian. I deducted from each observation the monthly mean corresponding to the solar time of the observation; thus eliminating the influence of the sun, considered as the cause of the regular diurnal variation. This deduction being made, the remainders represent the sum of all other influences; but as these numbers were sometimes + and sometimes — I augmented each by a

constant quantity. The numbers thus obtained were entered in the tables, each in the column corresponding to the distance of the moon from the magnetic meridian at the time of the observation. From the great number of observations that were in this combination, it might be expected that the effects of other influences would disappear, and that of the moon alone would be visible. The two horizontal elements, the declination and intensity, were thus treated, and the means obtained for the separate months were combined in a yearly mean, which is represented in the following table:—

I. Declination in Scale-divisions, each = $27''\cdot2261$.

Eastern Horary Angles.	Declin.	Declin.	Western Horary Angles.
12	11·44	11·21	11
13	10·96	10·79	10
14	11·14	11·01	9
15	10·78	10·64	8
16	10·76	10·24	7
17	10·55	10·69	6
18	10·51	10·42	5
19	10·52	10·15	4
20	10·66	10·16	3
21	10·48	10·32	2
22	10·54	10·46	1
23	10·97	10·62	0

From this table we may draw the following conclusions:—

I. If we take the sums of the declinations during the eastern and during the western horary angles, we find the first sum to exceed the second by $2\cdot60$ scale-divisions = $70''\cdot79$; therefore the declination is greater when the moon is east of the meridian, as already shown by the Milan observations.

II. If we compare the sum of the declinations corresponding to the horary angles from 6^h to 17^h with those from 18^h to 5^h , the first sum exceeds the second by $4\cdot40$ scale-divisions = $119''\cdot79$; the declination is therefore greater when the moon is in the neighbourhood of the inferior meridian: and the table shows that it is greatest at the hour when the moon is on the inferior meridian.

III. From the comparison of the sums of the declinations corresponding to the horary angles from 21^h to 2^h with the sums of those from 18^h to 20^h , and 3^h to 5^h , it results that the first sum exceeds the second by $0\cdot97$ scale-divisions = $26''\cdot41$; therefore it appears that at the time of the moon's passing the superior meridian, a second maximum of the declination

does take place, though considerably less than the one indicated above.

The results as to the horizontal intensity are contained in the following table :—

II. Horizontal Intensity.

Eastern Hor. Angles.	Intensity.	Intensity.	Western Hor. Angles.
12	32·92	34·15	11
13	32·62	33·33	10
14	32·78	32·63	9
15	33·64	32·52	8
16	32·62	33·12	7
17	32·03	31·53	6
18	31·11	31·90	5
19	30·96	32·16	4
20	30·16	31·77	3
21	29·45	29·96	2
22	29·07	30·31	1
23	29·92	30·04	0

From this table it appears :—

I. That the intensity is stronger when the moon is west of the magnetic meridian, for the sum of the intensities corresponding to western horary angles exceeds by 5·14 scale-divisions the sum of the intensities corresponding to eastern horary angles.

II. The intensity is decidedly greater when the moon is in the neighbourhood of the inferior meridian than when she is near the superior meridian, for the horary angles from 6^h to 17^h give a sum greater by 28·08 scale-divisions than do the horary angles from 18^h to 5^h.

7. The increased frequency and the greater number of the Prague observations enabled us to pursue the interesting phenomena of the magnetic perturbations with more exactness than we had been able to do at Milan. We proceeded in the following manner with both the horizontal elements. The changes were noted which had taken place between each observation and the next, and the sum of these (ζ) was taken for each day without reference to their signs. The monthly mean of these sums was taken and called Σ . Now if on any day the quotient $\frac{\zeta}{\Sigma}$ was found greater than 2, that day was

reckoned one of disturbance.

It hence appeared that the magnetic elements frequently sustain a very considerable change of brief duration; though

during the remaining hours of the day, the perturbations may so little exceed the average, that on the whole it may not be a day of disturbance according to the above definition.

This phænomenon, which may be called a *magnetic shock*, ought to be brought into notice, being, in fact, a perturbation of short duration, greater perturbations consisting only of several such shocks. As a definition of a magnetic shock, let g be the change in either of the magnetic elements between two successive observations, and σ the average change in the same month between every successive pair of observa-

tions; then every change for which $\frac{g}{\sigma} > 2$, is to be regarded as a magnetic shock.

The following table shows for each month the number of days of disturbance according to the above definition; and also the number of separate shocks which took place in addition, namely, on days which were not those of disturbance.

Table of Disturbances.

Month.	Declination.		Horizontal Intensity.	
	Days of Perturbation.	Shock.	Days of Perturbation.	Shocks.
1839. July.	1	1	5	2
Aug.	3	1	7	2
Sept.	5	4	4	10
Oct.	7	15	10	3
Nov.	8	7	16	...
Dec.	10	15	25	2
1840. Jan.	14	12	15	14
Feb.	7	8	9	...
March.	5	1	16	2
April.	2	5	11	2
May.	2	1	13	1
June.	1	1	9	6

From this table we may infer:—

I. That the perturbations are much more frequent in the winter than in the summer months; which may be caused, in part, by the force which produces the regular diurnal changes being much weaker in winter: but the very great perturbations which take place chiefly in the winter months, indicate that the disturbing forces have actually more intensity at that season of the year. The greatest disturbances observed in the twelvemonth occurred on the following days:—

4th and 15th of September.
 18th, 22nd, and 23rd of October.
 23rdof November.
 4th and 18th of January.
 6th, 7th, and 9th of February.
 29th and 30th of May.

II. The days of disturbance are more numerous in the horizontal intensity than in the declination.

III. In this year also several great perturbations were observed to occur on the same days on which the same phenomenon had taken place in preceding years. The days which particularly deserve notice in this respect, are about the 18th of January, from the 18th to the 22nd of February, and the 18th of October.

Great disturbances were observed in

1837. January 16th.	1837. February 18th.
1838. ——— 17th.	1838. ——— 16th and 21st.
1839. ——— 19th.	1839. ——— 18th and 21st.
1840. ——— 18th.	1840. ——— 18th and 21st.
1836. February 17th.	

In February, 1840, the disturbance of the horizontal intensity was but feebly marked, probably in consequence of the very great perturbations which took place in the same month on the 6th, 7th, and 9th.

The recurrence in October is a particularly marked one;

1836. October 18th.
1837. ——— 18th.
1838. ——— 17th.
1839. ——— 18th.

The two periods, February and October, are about equidistant from the winter solstice.

8. If we put together, without respect to signs, the shocks which occur at the different hours of the day, both on the perturbation days and on others, we obtain as a final result the following table, exhibiting the number of scale-divisions by which the needles were displaced by disturbances.

Sum of the Displacements.

Hours.	Declination.	Horizontal Intensity.
17 to 19	536·44	1373·36
20 to 22	463·90	1746·14
23 to 1	497·95	1425·33
2 to 4	697·76	1472·05
5 to 7	1475·16	1911·15
8 to 10	1481·17	2964·23

This table shows that the least disturbance takes place in the declination from 8 to 10 a.m., and the greatest from 8 to 10 p.m., a result which had already appeared from the Milan observations. In the horizontal intensity also the disturbances are more frequent in the evening than in the morning.

9. If we now take the signs into account, and call an augmentation of either element +, and a diminution —, the following table exhibits the remainders when the one sum is taken from the other.

Direction of the Changes.

Hours.	Declination.	Horizontal Intensity.
17 to 19	— 36·92	—1219·69
20 to 22	+224·56	—1193·38
23 to 1	+203·34	— 120·13
2 to 4	—405·36	— 104·75
5 to 7	—654·88	+ 94·37
8 to 10	—556·19	— 295·81

These numbers confirm for the declination the deduction already derived from the Milan observations, viz. that the declination is increased by the disturbances in the forenoon and middle of the day, and diminished by those occurring in the evening hours. In respect to the horizontal intensity, the negative signs are the prevailing ones, so that, in general, disturbances diminish this element, which is also in correspondence with the previous deductions; but it also appears from the numbers, that this occurs in a much higher degree during the hours of the night and morning than in the forenoon and afternoon.

10. During the greater disturbances we did not fail to observe for several hours, from 5 minutes to 5 minutes, and to study the march of the phænomena in all the elements with as much exactness as possible. Ten perturbations were thus observed, and the following circumstances were noticed as common to them all. These were before partly indicated by former observations, and appear to give a character of regularity to phænomena which at first sight might be regarded as wholly irregular.

I. Although the general effect of a disturbance is to weaken, on the whole, the horizontal intensity, considerable augmentations of that element do take place, but are of brief duration, and always preceding the diminution.

II. The horizontal intensity remains weaker for some time

after the great oscillations have ceased, and only gradually resumes its ordinary force.

III. All the alterations of this element are accompanied by changes of the dip, and may chiefly be ascribed to them; an increase of dip, and a diminution of the horizontal intensity always taking place together, and *vice versâ*.

IV. The vibration of the dipping-needle is more rapid during disturbances, consequently the total force is increased*; and as we have seen that the horizontal intensity is weakened at such times, the influence of the increase of dip preponderates over the augmentation of the total intensity. In countries where the dip is much less than at Prague this may not always be the case, and the horizontal intensity may increase during a disturbance.

V. The variations of the total intensity frequently occur simultaneously with those of the dip and horizontal force. In half the number of perturbations which were observed continuously, the strongest total intensity coincided with the highest dip and least horizontal intensity, or the weakest total intensity with the smallest dip and greatest horizontal intensity; affording a further evidence that the variations of the horizontal component are chiefly due to the changes of dip.

VI. In a strong disturbance all the three elements were usually affected, but in a variable degree, which may probably

* This is in contradiction to the 23rd paragraph in my letter to M. Kupffer, (Phil. Mag., April 1840, p. 249), and I hope that it may prove a rectification. I consider the method applied to the Milan observations an unsafe one, as by it the daily means of the times of vibration on days of disturbance were compared with the average time of the whole month. Such a comparison cannot show with exactness the small effect which the perturbations produce in this element, because in the course of a month it is subject to too great changes, whether real or instrumental. The greater frequency of the observations in each day at Prague, enabled me to proceed in a different manner, namely by comparing the mean of a disturbed day with those of the preceding and following days. Occasionally, indeed, the observations of the same day evidenced the nature of the change produced by the disturbance; the times observed before the commencement of the perturbation being sensibly longer than those which were determined during its continuance: thus the following times of vibration were observed on the 23rd of November.

Before the disturbance.				After it had commenced.			
h.	m.		s.	h.	m.		s.
3	30	Time of vibr.	= 12.1569	9	0	Time of vibr.	= 12.0724
4	30	—	= 12.1670	10	0	—	= 12.0894
6	0	—	= 12.1531	11	0	—	= 12.0752
8	0	—	= 12.1424	12	0	—	= 12.0624
				13	0	—	= 12.0901

The following day the time of vibration returned to its previous duration.

depend on the angle which the direction of the disturbing force makes with that of the regular force. By the hourly observations, the 23rd of March appeared to be a day of greater disturbance than any other in the month in respect to the horizontal intensity, although not a single shock was shown by the declination magnetometer. Had, however, the observations been continuous instead of hourly, the declination might also have been seen to have been disturbed.

VII. The more these phænomena are studied the more strong becomes the impression of the importance of observing them at short intervals. Those of 5 minutes are too long, for the bar may alter its position many hundred scale-divisions in that interval, as we found by the Prague bifilar on the 22nd of October. On days of great disturbances, or when the aurora borealis is seen, we observe the two horizontal elements, here and at Göttingen, uninterruptedly for several hours at intervals of 15 or 20 seconds: it is much to be desired that this practice should become more general.

In my letter to M. Kupffer, I noticed certain vertical vibrations supposed to be produced by earthquakes. By a mistake of the pen these were said to have taken place in the dipping-needle, whereas it was in the declination magnetometer that they occurred. In Prague this instrument is suspended to a beam, which is supported by the principal walls of the house, and such vibrations only take place during very violent winds. In such cases there can be no doubt that they are due to this mechanical cause.

In addition to the magnetic observations, we now note the temperature of the earth from one to five feet deep, and the height and temperature of the Moldau. I wish to add observations on the temperature of the dew-point and the intensity of the sun's rays, and shall be much obliged to you to inform me the price of the actinometer and the dew-point hygrometer, that I may request our government to purchase these instruments.

I remain, with the highest esteem, &c. &c.

Major Sabine, R.A.

CARL KREIL.

LXII. *On Mr. Potter's Application of Huyghens's Principle in Physical Optics.* By JOHN TOVEY, Esq.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN your October Number, p. 243, Mr. Potter gives some examples of the application of the principle above-named; from which he contends that the result of this principle is

“that light ought to bend into the shadows of bodies to an indefinite extent, as sound is known to pass through all apertures and to bend round all obstacles.” Consequently, “that the result Mr. Airy (Tracts, p. 270,) has obtained by an approximate method is not to be depended upon, and that the objection to the undulatory theory, which was believed to have been removed, remains in full force.” p. 246.

I thought it possible that Mr. Airy might reply to this, but as he has not yet done so, I beg to be allowed to offer a few words, in order to point out the mistake which I conceive Mr. Potter has made.

The expressions, which Mr. Potter obtains for the intensity of the light, have reference only to a certain line *CB*. He has not shown that there ought to be any light in the shadow except on this line. But a luminous line is merely a geometrical conception, from which no inference can be drawn as to the sensible intensity of the light in the shadow where this line is found.

It appears then that Mr. Potter has mistaken a luminous *line* for a luminous *space*; and, consequently, that his conclusions have, in reality, no foundation.

The meaning of what I have here stated may be illustrated by a fact which Mr. Potter has mentioned. The intensity of the light at certain points on a line in the centre of the shadow of a circular disc is, according to the theory, the same as if the light passed uninterruptedly. Now, when the disc is very small, and the light properly managed, a luminous spot may be observed in the centre of the shadow: when the disc is larger the spot vanishes; not from the diminution of its brightness, but from the diminution of its magnitude.

I am, Gentlemen, yours, &c.

Littlemore, Clitheroe, Nov. 5, 1840.

JOHN TOVEY.

LXIII. *On the Focal Properties of Surfaces of the Second Order.* By JAMES BOOTH, A.M., of Trinity College, Dublin; Principal of and Professor of Mathematics in Bristol College.

WHETHER properties of surfaces of the second degree exist, analogous to those of the foci of the conic sections, has long been a subject of inquiry with the most distinguished geometers; among whom may be mentioned as pre-eminent in researches of this nature M. Chasles and Professor MacCullagh, who have arrived independently at a series of discoveries, of which may be stated as the most important and fundamental, the property, “that in the prin-

principal planes of a surface of the second order, there exist conic sections, termed by him *eccentric conics*, confocal to the sections of the surface in the principal planes; possessing properties analogous to the foci of curves of the second order; and cylinders perpendicular to those planes, bearing an analogy to the directrices of the same curves;” but this and other theorems of the same class, may be considered rather as the limiting relations of confocal surfaces, than as analogous to the well-known properties of curves of the second degree; and this view of the subject is further confirmed by the consideration, that the theorems above alluded to fail in the very case most analogous to that of the conic sections, when the surface is one of revolution round the transverse axe.

M. Chasles, indeed, in a very elegant memoir published now more than ten years ago *, has given several of the analogous theorems in the case of surfaces of revolution round the transverse axe, but has not hitherto extended his researches to the case of oblate spheroids, or to that of surfaces having three unequal axes, to do which is the object of the following pages.

By a simple application of a new method, which has now been for some time published †, I have been led to the discovery of a very extensive class of properties of surfaces of the second degree, hitherto, I believe, entirely unknown; from which may be easily deduced a series of theorems, relative to curves of the second order, none of which, so far as I am aware, have been yet given to the public.

The restricted limits of the present communication preclude the possibility of giving more than an outline of the theory, and the enunciations of several new theorems, the demonstrations of several of which being somewhat tedious, have been suppressed; but this can cause no difficulty to any moderately expert analyst.

(1.) Let a, b, c , denote the three semiaxes in the order of magnitude of a surface of the second order, which for brevity may be represented by the symbol (Σ) , and let the eccentricities of the three principal sections of the surface (Σ) be e, ε, η , so that

$$e^2 = \frac{a^2 - b^2}{a^2}; \quad \varepsilon^2 = \frac{a^2 - c^2}{a^2}; \quad \eta^2 = \frac{b^2 - c^2}{b^2}.$$

(2.) Let u denote the semidiameter of the surface passing

* See the *Nouveaux Mémoires de l'Académie Royale de Bruxelles*, vol. v.

† See a short treatise by the author, “On the application of a new Analytic Method to the theory of Curves and Curved Surfaces.” Dublin, Hodges and Smith, 1840.

through one of its *umbilici*, and let a point be assumed on this diameter, at the distance $u \varepsilon$ from the centre; this point may be termed a *focus* of the surface. Hence in general a surface of the second order has *four* FOCI, situated on the umbilical diameters.

(3.) Let a plane be drawn parallel to one of the circular sections of the surface, meeting the umbilical diameter conjugate to this circular section, at the distance $\frac{u}{\varepsilon}$ from the

centre; this plane may be termed a *directrix plane* of the surface.

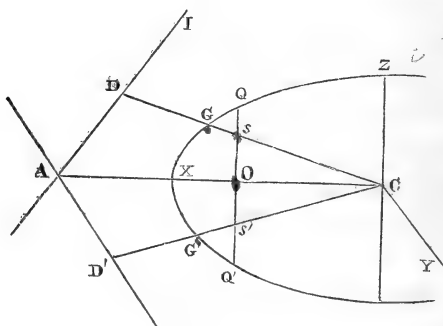
This plane, and the focus of the surface, on the diameter of the surface, conjugate to this plane, are *polar plane* and *pole*, relative to the given surface. Hence in general a surface (Σ) has *four* DIRECTRIX planes, parallel two by two, to its circular sections.

Each pair of those planes intersect in the directrices of the principal section, whose semiaxes are a and b ; and these pairs may be termed *conjugate directrix planes*.

The foci of the surface, which are the poles of the conjugate directrix planes, may be called *conjugate foci*.

The line joining a pair of conjugate foci is perpendicular to the plane of the principal section in the plane of xy , whose semiaxes are a and b .

(4.) Let the middle point of this line be called the *focal centre* of the surface. Hence in general a surface of the second order has *four* FOCI and *two* FOCAL CENTRES.



Thus C is the centre of (Σ).

G and G' are the umbilici, s and s' the conjugate foci of the surface; A D, A D', the conjugate directrix planes; O the focal centre of (Σ); or the focus of the principal section of

(Σ) in the plane of xy , $CG = u$, $CX = a$, $CZ = c$, $CY = b$, $CD = \frac{u}{\epsilon}$, $Cs = u\epsilon$, $CO = ae$, $CA = \frac{a}{e}$.

(5.) Let $\theta = DAC$, be the angle which a directrix plane (or which a circular section of the surface as being parallel to it) makes with the plane of xy , or with the plane of the principal section, whose semiaxes are a and b ; then

$$\cos \theta = \frac{\eta}{\epsilon}.$$

(6.) Let ω be the angle which the umbilical diameter u makes with the corresponding directrix plane, then

$$\tan^2 \omega = \tan^2 CDI = \frac{a^2 c^2}{(a^2 - b^2)(b^2 - c^2)}; \text{ or } \sin \omega = \frac{ac}{bu}.$$

Hence in two cases, the directrix plane is perpendicular to the diameter conjugate to it; either when the surface is one of revolution round the transverse axe, when $b = c$, or when (Σ) is an oblate spheroid, in which case $a = b$.

When the surface is an elliptic paraboloid, let l and l' be the semiparameters of the parabolas in the planes of xy and xz ; then $\theta = \omega$, and $\sin^2 \omega = \frac{l'}{l}$.

(7.) When (Σ) is a surface of revolution round the transverse axe, or the axis of X , $\eta = 0$; and in this case the conjugate directrix planes AD , AD' coincide and become perpendicular to the transverse axe CX , at the distance $\frac{a}{e}$ from the centre; and the foci of the surface coincide with the focal centre; but when (Σ) is an oblate spheroid $e = 0$, and $\epsilon = \eta$; in this case then $\theta = 0$, or the conjugate directrix planes become parallel to the plane of xy , distant from it by $\frac{c}{\epsilon}$; and the focal centre coincides with the centre of the surface.

(8.) When the surface is an elliptic paraboloid, one pair of conjugate directrix planes is infinitely distant, so that this surface has but *two* foci, and *two* directrix planes.

(9.) When the surface is a cone, the conjugate directrix planes pass through the vertex, and are parallel to the circular sections of the cone; the foci of the surface, and the focal centres all coincide with the vertex.

(10.) The distance of a focus of the surface (Σ) from the focal centre, or from the plane of xy , is $= \frac{c b \eta}{a}$.

(11.) The line $OQ = \frac{cb}{a}$, or the cord of (Σ) passing through a pair of conjugate foci $= \frac{2cb}{a}$.

(12.) The length of the perpendicular from one of the foci of the surface on the corresponding directrix plane, is $= \frac{c^3}{ab\epsilon}$.

(13.) The length of the perpendicular from the centre on one of the directrix planes $= \frac{ac}{b\epsilon}$.

(14.) The segment of the cord joining a pair of conjugate foci, intercepted between the plane of xy and one of the directrix planes, is $= \frac{cb}{a\eta}$.

(15.) The length of the perpendicular from the focal centre on one of the conjugate directrix planes $= \frac{bc}{a\epsilon}$.

We now proceed to give the enunciations of a very few theorems, merely as specimens of the results which flow from the preceding definitions, and the application of the method alluded to above; premising that neither the preceding definitions, nor the following theorems, are applicable either to the hyperboloid of one sheet, or to the hyperbolic paraboloid; and this may suggest a natural division of surfaces of the second order into two classes, the one containing the umbilical surfaces, the other those surfaces whose generatrices are right lines.

PROP. I.—From any point τ of a surface of the second order, let perpendiculars p, p' , be let fall on two conjugate directrix planes; the rectangle under those perpendiculars is to the square of r ,—the distance of the point τ from the focal centre O , relative to those conjugate directrix planes in a constant ratio, as the square of the perpendicular P from the centre on one of the directrix planes is to the square of the transverse axe a , or

$$\frac{pp'}{r^2} = \frac{P^2}{a^2};$$

this constant ratio is one of equality, when the least semiaxis c of the surface is equal to the perpendicular let fall from the centre of the surface on the line joining the extremities of the semiaxes a and b : this ratio of equality can never exist then when the surface is one of revolution round the transverse axes, except when (Σ) is an elliptic paraboloid.

Generally when the ratio is one of equality, $\epsilon^2 + \eta^2 = 1$.

When the surface is one of revolution round the transverse axis the conjugate directrix planes coalesce, p and p' therefore coincide, and are equal, and $P = \frac{a}{e}$: hence $\frac{p}{r} = \frac{1}{e}$;

which is a fundamental property of surfaces of revolution and of the conic sections.

When the surface is an oblate spheroid the conjugate directrix planes become parallel to the plane of xy , and the focal centre coincides with the centre of the surface, the perpendiculars pp' are in the same right line but on opposite sides of τ , hence in an oblate spheroid, if a right line is drawn perpendicular to the directrix planes meeting the surface in τ , and the directrix planes in m and m' ; the rectangle $m\tau \times \tau m'$ is to the square of the semidiameter $O\tau$, as the square of the distance between the directrix planes is to the square of the diameter of the central circular section, or

$$\frac{m\tau \times \tau m'}{r^2} = \frac{m^2}{4a^2}.$$

When the surface is an elliptic paraboloid, let l and l' be the semiparameters of the parabolas in the planes of xy and xz , then

$$\frac{pp'}{r^2} = \frac{l'}{l}.$$

When the surface is a cone, let α and β denote its semiangles; $\alpha > \beta$; then

$$\frac{pp'}{r^2} = \frac{\sin^2 \beta}{\tan^2 \alpha},$$

or the square of the distance of any point on the surface of a cone from the vertex is to the rectangle under the perpendiculars from the same point on two planes passing through the vertex parallel to the circular sections of the cone in a constant ratio.

PROP. II. — The cone whose vertex is a focal centre of (Σ) , and base any plane section of this surface, has its circular sections parallel to the planes passing through the vertex of the cone, and the right lines in which the base of the cone intersects the conjugate directrix planes.

When the base of the cone passes through the right line, in which the conjugate directrix planes intersect, the planes parallel to the circular sections coincide, and the cone is therefore a surface of revolution.

Hence the cone whose vertex is a focal centre of (Σ) , and base any plane section of this surface, passing through the intersection of the conjugate directrix planes, is a surface of

revolution, its axis being the line joining the conjugate foci of the surface.

When (Σ) is a surface of revolution round the transverse axe, the conjugate directrix planes coincide, therefore the planes through the vertex of the cone and the intersections of the directrix planes by its base, coincide, hence the cone is a right cone, and we obtain the known theorem, that "the cone whose vertex is the focus, and base any plane section of a surface of revolution, round the transverse axe, is a right cone."

Hence also the cone whose vertex is the centre of an oblate spheroid and base any plane section of this surface, has its circular sections parallel to the diametral planes passing through the right lines in which the base of the cone intersects the parallel directrix planes.

PROP. III.—The preceding theorem may be generalized thus: let a cone enveloping (Σ) , having its vertex anywhere on the line $Q Q'$ joining the conjugate foci, be cut by any plane passing through the intersection of the conjugate directrix planes in a conic section, the cone whose base is this section and vertex the focal centre of the surface, is a surface of revolution.

PROP. IV.—Let a right line be drawn meeting a surface of the second order, in the points τ and τ' , and the conjugate directrix planes in the points m and m' , the segments of this right line, $m\tau$ and $m'\tau'$, subtend equal angles at the focal centre.

When the line is parallel to one of the directrix planes, it may be easily shown that the rectangle $m\tau \times m\tau' = \overline{mO}^2$, O being the focal centre, and m the point in which the right line meets the directrix plane to which it is *not* parallel.

Hence, if a tangent plane is drawn to a surface of the second order at the umbilicus, meeting the other conjugate directrix plane in a right line, the distances of any point in this right line from the umbilicus and focal centre are equal.

When the surface is one of revolution round the transverse axe the points m and m' coincide, or the line Om bisects the supplement of the angle $\tau O \tau'$, which is a known property of such surfaces.

Let a series of surfaces of the second order, having the same focal centre, and the same pair of conjugate directrix planes, be cut by any transversal; the segments of this right line, between each pair of surfaces, subtend equal angles at the common focal centre.

When (Σ) is a cone, if from m any point in one of the directrix planes a right line is drawn parallel to the other,

meeting the cone in the points τ, τ' , the rectangle $m\tau \times m'\tau' =$ square of the distance of m from the vertex of the cone. Hence if a sphere is described through the circular base and vertex of a cone, the tangent plane to the sphere at the vertex of the cone is parallel to the second circular section of the cone.

PROP. V.—From the four foci of a surface of the second order, let fall perpendiculars on a tangent plane; multiplying together the perpendiculars from those foci which are situated on the same diameter and taking the sum, we have

$$Pp + P'p' = 2 \left\{ b^2 \sin^2 \nu + \frac{c^2 u^2}{a^2} \cos^2 \nu \right\}, \nu \text{ denoting the}$$

angle the perpendiculars make with the axis of Z .

When (Σ) is a surface of revolution round the transverse axis $P = P'$, $p = p'$, $b = c$ and $u = a$; hence $Pp = b^2$.

When the surface is an elliptic paraboloid, P and P' are infinite, and we obtain

$$(p + p') \cos \lambda = l \sin^2 \nu + l' \cos^2 \nu.$$

λ being the angle the perpendicular makes with the axis of X .

PROP. VI.—Through any point of a surface of the second order, let two cords be drawn, passing through the extremities of the cord of the surface Q, Q' , which joins a pair of conjugate foci, and meeting one of the conjugate directrix planes in the points m, m' , these points m, m' , subtend at the focal centre a right angle.

PROP. VII.—Let a plane quadrilateral be inscribed in a surface of the second order, whose sides, $\alpha, \beta, \gamma, \delta$, are produced to meet one of the directrix planes in the points A, B, C, D . The sum of the angles which the points A, B and C, D subtend at the focal centre is equal to two right angles.

Let two of the sides α, β of the quadrilateral be fixed, and let γ, δ be variable; then A and B are fixed, and therefore the angle $A O B$ is constant; hence also the angle $C O D$ is constant.

DEF.—The right line in which two tangent planes intersect, and the line joining the points of contact, are called *conjugate polars* relative to the given surface.

PROP. VIII.—Two conjugate polars to a surface (Σ) , meet one of the directrix planes in two points, which subtend a right angle at the focal centre.

When the conjugate polars become conjugate tangents, the proposition still holds, and when the conjugate tangents are tangents to the lines of curvature they are at right angles. Hence if a tangent plane be drawn to any point of a surface,

meeting one of the directrix planes in a right line, and the tangents to the lines of curvature be produced to meet this line in the points m, m' , the sphere described on mm' as diameter will pass through the point of contact and the focal centre; hence may be given a new method of determining the lines of curvature.

PROP. IX.—Through the focal centre of a surface (Σ) , let a right line and a plane be drawn perpendicular to each other; the line meeting the surface in a point τ , and the plane meeting one of the directrix planes in a right line mm' ; the plane $mm'\tau$ envelopes a surface of revolution, whose focus is the focal centre of (Σ) , and whose directrix plane passes through the intersection of the conjugate directrix planes of the given surface.

Let the equation of the surface, the origin being placed at the focal centre, be

$$\frac{x'^2}{a^2} + \frac{y'^2}{b^2} + \frac{z'^2}{c^2} - \frac{2ex'}{a} = \frac{b^2}{a^2} \quad (a); \quad (x' y' z') \text{ being the}$$

coordinates of the point τ ; let $\xi = \mu \zeta + \alpha$, (b) $v = \nu \zeta + \beta$, (b') be the* tangential equations of the line mm' in the directrix plane; and $x'\xi + y'v + z'\zeta = 1$ (c) the equation of the plane passing through the point $(x' y' z')$ and the right line mm' .

In the first place, as the line mm' is in a plane passing through the origin, $\mu = \frac{x'}{z'}$, $\nu = \frac{y'}{z'}$; and equations (b) (b') are changed into $\xi = \frac{x'}{z'} \zeta + \alpha$, $v = \frac{y'}{z'} \zeta + \beta$; and as the line mm' is in the directrix place of which the tangential co-ordinates are

$$\xi = -\frac{ae}{b^2}, \quad v = 0, \quad \zeta = \frac{a\eta}{bc}; \quad \text{these values of } \xi, v, \zeta$$

must satisfy the tangential equation of the right line mm ; by these substitutions the equations (b) (b') are changed into

$$\left. \begin{aligned} z'c(b^2\xi + ae) &= x'(b^2c\xi - ab\eta) \\ z'cb^2v &= y'(b^2c\xi - ab\eta) \end{aligned} \right\} \quad (d)$$

and we have now to eliminate $x' y' z'$ between the four equations (a) (c) (d); from the three latter we get

$$\begin{aligned} x' &= \frac{b^2c^2\xi - ac^2e}{b^2c^2(\xi^2 + v^2 + \zeta^2) + ac^2e\xi - ab\eta\zeta} \\ y' &= \frac{b^2c^2v}{b^2c^2(\xi^2 + v^2 + \zeta^2) + ac^2e\xi - ab\eta\zeta} \end{aligned}$$

* See the treatise quoted above, page 11.

$$z' = \frac{b^2 c^2 \zeta - a b c \eta}{b^2 c^2 (\xi^2 + v^2 + \zeta^2) + a c^2 e \xi - a b c \eta \zeta}.$$

Putting those values of $x' y' z'$ in (a), and making

$V = b^2 c^2 (\xi^2 + v^2 + \zeta^2) + 2 a c^2 e \xi - a^2 \eta^2 - c^2$,
we shall find for the resulting equation
 $b^4 c^2 a \{ 2 b c \eta \zeta - c^2 e \xi - a \epsilon^2 \} V = 0$, which is satisfied by
putting $V = 0$, or $b^2 c^2 (\xi^2 + v^2 + \zeta^2) + 2 a c^2 e \xi = a^2 \eta^2 + c^2$,
the tangential equation of an ellipsoid of revolution whose
focus coincides with the origin. It is easily shown that the
tangential equation of the given surface referred to the same
origin and axes is $b^2 (\xi^2 + v^2) + c^2 \zeta^2 + 2 a e \xi = 1$.

Hence if the given surface is a surface of revolution $\eta = 0$,
and the locus found becomes identical with the given sur-
face, as is otherwise known.

When the given surface is an oblate spheroid $e = 0$, $a = b$,
and the locus becomes $c^2 (\xi^2 + v^2 + \zeta^2) = 1$, the tangential
equation of a sphere described on the axis of revolution of
the oblate spheroid as diameter.

Similarly may it be shown, that if through any fixed point
in space, a right line and a plane are always drawn at right
angles to each other; the former meeting a fixed plane in
a point τ , and the second intersecting another fixed plane in
a right line $m m'$; the plane $m m' \tau$ envelopes a surface of re-
volution of the second order, one of whose foci is at the fixed
point.

In a future Number, after treating of the general and
numerous kindred properties of the two surfaces of the se-
cond order, whose generatrices are right lines, the author
proposes resuming this subject, and developing briefly a
general method, by the theory of *reciprocal polars*, of de-
monstrating these and other similar theorems, many of which
want of space has compelled him to omit in the present com-
munication.

LXIV. *Address of the General Secretaries of the British Association*, RODERICK IMPEY MURCHISON, *F.R.S., F.G.S.*, and
Major EDWARD SABINE, *V.P.R.S.*: read at the Meeting
at Glasgow, September 1840.

IN entering upon the duty assigned to us, we heartily con-
gratulate our associates on this our second assembly in
Scotland. As on our first visit we were sustained by the in-
tellectual force of the metropolis of this kingdom, so now, by
visiting the chief mart of Scottish commerce, and an ancient

seat of learning, we hope to double the numbers of our northern auxiliaries.

Supported by a fresh accession of the property and intelligence of this land, we are now led on by a noble Marquis, who, disdaining not the fields we try to win, may be cited as the first Highland chieftain who, proclaiming that knowledge is power, is proud to place himself at the head of the clans of science.

If such be our chief, what is our chosen ground?—raised through the industry and genius of her sons, to a pinnacle of commercial grandeur, well can this city estimate her obligations to science! Happily as she is placed, and surrounded as she is by earth's fairest gifts, she feels how much her progress depends upon an acquaintance with the true structure of the rich deposits which form her subsoil; and great as they are, she clearly sees that her manufactures may at a moment take a new flight by new mechanical discoveries. For she it is, you all know, who nurtured the man whose genius has changed the tide of human interests, by calling into active energy a power which (as wielded by him), in abridging time and space, has doubled the value of human life, and has established for his memory a lasting claim on the gratitude of the civilized world. The names of Watt and Glasgow are united in imperishable records!

In such a city, then, surrounded by such recollections, encouraged by an illustrious and time-honoured university, and fostered by the ancient leaders of the people, may we not augur that this Meeting of the British Association shall rival the most useful of our previous assemblies, and exhibit undoubted proofs of the increasing prosperity of the British Association?

Not attempting an analysis of the general advance of science in the year that has passed since our meeting at Birmingham, we shall restrict ourselves, on the present occasion, to a brief review of what the British Association has directly effected in that interval of time, as recorded in the last published volume of our Transactions. From this straight path of our duty we shall only deviate in offering a few general remarks on subjects intimately connected with the well-being and dignity of our institution.

One of the most important—perhaps the most important service to science—which it is the peculiar duty of the Association to confer, is that which arises from its relation to the Government,—the right which it claims to make known the wants of science, and to demand for them that aid which it is beyond the power of any scientific body to bestow. In the

fulfilment of this important and responsible duty, the Association has continued to act upon the principle already laid down in the Address of the General Secretaries at the meeting at Newcastle in 1838, namely, to seek the aid of Government in no case of doubtful or minor importance; and to seek it only when the resources of individuals, or of individual bodies, shall have proved unequal to the demand. The caution which it has observed in this respect has been eminently displayed in the part which it has taken with reference to the Antarctic expedition, and to the fixed magnetical observatories. It abstained from recommending the former to the Government until it had called for, and obtained from Major Sabine, by whom the importance of such an expedition was first urged, a report in which that importance was placed beyond all doubt; and it withheld from urging the latter, although its necessity was fully felt by some of its own members, until the letter of Baron Humboldt to the Duke of Sussex gave authority and force to its recommendation.

The delay which has in consequence occurred, has been productive of signal benefit to each branch of this great two-fold undertaking. Since the time alluded to, our views of the objects of investigation in terrestrial magnetism have been greatly enlarged, at the same time that they have become more distinct. Major Sabine's memoir on the Intensity of Terrestrial Magnetism has served to point out the most interesting portion of the surface of the globe, as respects the distribution of the magnetic force, and has indicated, in the clearest manner, what still remained for observation to perform; and the beautiful theory of M. Gauss, which has been partly built upon the data afforded by the same memoir,—while it has assigned the most probable configuration of the magnetic lines of declination, inclination, and intensity,—has done the same service with respect to all the three elements.

In another point of view, also, delay has proved of great value to both branches of the undertaking, but more especially to the fixed observatories. Our means of instrumental research have, since the time of their first projection, received great improvements, as well in their adequacy to the objects of inquiry, as in their precision; and finally, the two great lines of inquiry,—the research of the distribution of Terrestrial Magnetism on the earth's surface,—and the investigation of its variations, secular, periodic, and irregular,—have been permitted to proceed *pari passu*.

Last of all, the prudent caution, and vigilant care, which the two great scientific bodies have exhibited, both in the origin and progress of the undertaking, have naturally in-

spired the Government with confidence; and while on the one hand science has not hesitated to demand of the country all that was requisite to give completeness to a great design, so on the other, the Government of the country has not hesitated to yield, with a liberal and unsparing hand, every request the importance of which was so well guaranteed.

But while we thus enumerate the benefits which have resulted to magnetical science from the delay, it must be also acknowledged that something has been lost also, not to science, but to British glory. Although terrestrial magnetism stood forward as the prominent object of the Antarctic expedition, yet it was also destined to advance our knowledge of the "*physique du globe*," in all its branches, and especially in that of geography. Had the project of an Antarctic expedition been acceded to when it was first proposed, viz. at the meeting of the British Association, in Dublin, in 1835, there can be no reasonable doubt, that a discovery, which by its extent may almost be designated a Southern Continent, situated in the very region to which its efforts were to have been chiefly directed, must have fallen to its lot; and the flag of England been once more the first to wave over an unknown land. But while, as Britons, we mourn over the loss of a prize which it well became Britain and British seamen to have made their own, it is our part too as Britons, as well as men of science, to hail the great discovery—one of the very few great geographical discoveries which remained unmade;—and to congratulate those by whom it has been achieved, those whom we are proud to acknowledge as fellow-labourers, and who have proved themselves in this instance our successful rivals in an honourable and generous emulation.

The caution which has characterized the British Association in the origination of this great undertaking, has been followed up by the Royal Society in the manner in which it has planned the details, and in the vigilant care with which it has watched over the execution. Of the success which has attended this portion of the work, the strongest proof has been already given in the unhesitating adoption of the same scheme of observation by many of the continental observers, and in the wide extension which it has already received in other quarters of the globe. All that yet remains is to provide for the speedy publication of the results. The enormous mass of observations which will be gathered in, in the course of three years, by the observatories established under British auspices, and by the Antarctic expedition, will render this part of the task one of great expense and labour. To meet the former, we must again look to the Government, and to the East India

Company, who will certainly not fail to present the result of their munificence to the world in an accessible form. The latter can only be overcome by a well-organized system. The planning of this system, will, of course, be one of the first duties of the Royal Society; and it is important that it should be so arranged, that while every facility in the way of reduction may be given to those who shall hereafter engage in the theoretical discussion of the observations, care is taken at the same time that the data are presented entire, without mutilation or abridgement. The Council of the Royal Society, will, doubtless, be greatly assisted in this duty by the eminent individual who has had in every way so large a share in the formation of these widely scattered magnetic establishments, and whose own observatory, founded by the munificence of the Dublin University, has nearly completed a twelve months' magnetic observations on that enlarged and complete system of which it set the first example.

In referring, as we have done, to those most valuable services which the Royal Society have rendered, and are continuing to render, in directing and superintending the details of this great undertaking, in both its branches, it is right that, on the part of the British Association, we should express the cordial satisfaction and delight with which we have witnessed their exertions, united with our own in this common cause; nor should we omit to recognize how much this desirable concurrence has been promoted by the influence of the noble president of the Royal Society, the Marquis of Northampton, whom, as on so many former occasions, we have the pleasure of seeing amongst us, as one of our warmest supporters and most active members.

In the volume of our Transactions now under notice, is contained the memorial presented to Lord Melbourne by the Committee of the British Association, appointed to represent to Her Majesty's Government the recommendations of the Association on the subject of terrestrial magnetism. This memorial is one of many services which have been rendered to our cause by Sir John Herschel, whose name, whose influence, and whose exertions, since our meeting two years since at Newcastle, have largely contributed to place the subject where it now stands. The devoted labour of other of our members has long been given to an object which they have had deeply at heart, viz. the advancement of the science of terrestrial magnetism; but the sacrifice which Sir John Herschel has made of time, diverted from the great work, in which his ardent love of astronomy, his own personal fame, and his father's memory are all deeply concerned, the more urgently demands

from our justice a grateful mention, because the science of magnetism had no claim on him, beyond the interest felt in every branch of science, by one to whom no part of its wide field is strange, and the regard which a national undertaking such as this deserved, from the person who occupies his distinguished station amongst the leaders of British science.

The advancement of human knowledge, which may be reckoned upon as the certain consequence of the Antarctic expedition (should Providence crown it with success), and of the arrangements connected with it, is of so extensive a nature, and of such incalculable importance, that no juster title to real and lasting glory than it may be expected to confer, has been earned by any country at any period of time; nothing has ever been attempted by England more worthy of the place which she occupies in the scale of nations. When much which now appears of magnitude in the eyes of politicians has passed into insignificance, the fruits of this undertaking will distinguish the age which gave it birth, and, engraved on the durable records of science, will for ever reflect honour on the scientific bodies which planned and promoted it, and on the Government which, with so much liberality, has carried it into effect.

Were the value of this Association, Gentlemen, to be measured only by the part which it has taken in suggesting and urging this one object, there might here be enough to satisfy the doubts of those who question its utility: to overlook such acts as these, and the power of public usefulness which they indicate, to scrutinize with microscopic view the minute defects incidental to every numerous assemblage of men, to watch with critical fastidiousness the taste of every word which might be uttered by individuals amongst us, instead of casting a master's eye over the work which has been done, and is doing, at our meetings, is no mark of superior discernment and comprehensive wisdom, but is evidence rather of a confinement to narrow views, and an indulgence of vain and ignoble passions.

But to proceed with our useful efforts,—one of the principal objects of our Annual Volumes, is the publication in the most authentic form of the results of special researches, undertaken by the request, and prosecuted in many instances at the cost, of the Association. It is a trite remark, that if a man of talent has but fair play, he will soon secure to himself his due place in public estimation. We fully admit the truth of this in many instances, and above all where the points of research are connected with commerce and the useful arts; but many also are the subtle threads of knowledge, which,

destined at some future day to be woven into the great web in which all the sciences are knit together, are yet not appreciable to the vulgar eye, and if simply submitted to public judgement, would too often meet with silent neglect. Numberless, we say, are the subjects (and if your Association exceeds a centenary, still more numerous will they be) with which the retired and skilful man may wish to grapple, and still be deterred by his want of opportunity or of means. Then is it that, adopting the well-balanced recommendations of the men in whose capacity and rectitude you confide, you step forward with your aids, and bring about these recondite researches, the result of which in the volume under our notice, we now proceed to consider.

The first of these inquiries to which we advert, you called for at the hands of Professor Owen, upon "British Fossil Reptiles," one of the branches of Natural History, on a correct knowledge of which the development of geology is intimately dependent.

The merits of the author selected for this inquiry are now widely recognized, and he has, with justice, been approved as the worthy successor of John Hunter, that illustrious Scotchman who laid the foundation of comparative anatomy in the British isles. That this science is now taking a fresh spring, would, we are persuaded, be the opinion of Cuvier himself; could that eminent man view the progress which our young countryman is making towards the completion of the temple of which the French naturalist was the great architect. It is therefore a pleasing reflection, that when we solicited Professor Owen to work out this subject, we did not follow in the wake of Europe's praise, but led the way (as this Association ought always to do), in drawing forth the man of genius and of worth; and the value of our choice has been since stamped by the approval of the French Institute.

If Englishmen* first perceived something of the natural affinities of Palæosaurians, it was reserved for Cuvier to complete all such preliminary labour. The publication of his splendid chapters on the Osteology of the Crocodile and other Reptiles, drew new attention and more intelligent scrutiny to these remains; and it ought to be a subject of honest pride to us to reflect that the most interesting fruits of the researches of that great anatomist were early gathered by the English Palæontologists, Clift and Hume. One of our leaders, whose report on Geology ornaments the volumes of this Association, formed the genus *Plesiosaurus*, on an enlarged view of the relation subsisting between the ancient and modern forms

* Stukeley.

of reptile life; while shortly after Buckland established the genus *Megalosaurus*, and Mantell, *Iguanodon* and *Hylæosaurus*, worthy rivals of the *Geo-Sauri* and *Moso-Sauri* of Cuvier. The other Englishmen who have best toiled in this field, are De la Beche, Hawkins, and Sir Philip Egerton.

Yet although this report is on *British* reptiles, we are fully alive to the great progress which this department has made, and is making, on the Continent, through the labours of Count Münster, Jäger, and Hermann Von Meyer. The last-mentioned naturalist has been for some time preparing a series of exquisite drawings of very many forms unknown to us in England, most of which have been detected in the Muschelkalk, a formation not hitherto discovered in the British isles. Yet despite of all that had been accomplished in our own country or elsewhere, Professor Owen has thrown a new light of classification on this subject, founded on many newly discovered peculiarities of osseous structure, and has vastly augmented our acquaintance with new forms, by describing sixteen species of *Plesiosauri*, three of which only had been recognisably described by other writers; and ten species of *Ichthyosauri*, five of which are new to science. Such results were not to be obtained without much labour; and previous to drawing up his report, Professor Owen had visited the principal depositories of *Enaliosauri* described by foreign writers, as well as most of the public and private collections of Britain. This, the first part of Mr. Owen's report, concludes with a general review of the geological relations and extent of the strata through which he has traced the remains of British *Enaliosauri*. The materials which he has collected for the second and concluding portion of his report on the terrestrial and crocodilean Sauria, the Chelonia, Ophidian, and Batrachian reptiles, are equally numerous, and the results of these researches will be laid before the Association at our next meeting. Deeply impressed as we are with the value of this report, we cannot conclude a notice of it, without again alluding to its origin, in the words of Professor Owen himself. "I could not," says he, "have ventured to have proposed to myself the British Fossil Reptilia as a subject of continuous and systematic research, without the aid and encouragement which the British Association has liberally granted to me for that purpose."

Mr. Edward Forbes, whose labours in detecting the difference of species and varieties among the existing marine testacea of our shores, have been most praiseworthy, has on this occasion given us a report on the pulmoniferous mollusca of the British isles. The variations in the distribution of the

species in this class of animals, are shown by him to depend both upon climate and upon soil, the structure of the country (or geological conditions) having quite as much share in such varied distribution, as the greatest diversity of temperature. The Association has to thank the author for valuable tables, which show both the distribution of the pulmoniferous mollusca in our islands, and their relations to those of Europe generally.

[To be continued.]

LXV. *Remarks on the Electricity of Steam.* By Dr.
CHARLES SCHAFHAEUTL*.

THE discovery of a large quantity of free electricity in a jet of steam† is decidedly of great interest, but the circumstances under which this electricity is developed, are still involved in such great mystery, that I cannot omit to call the early attention of the experimenter to some points which appear to me of primary importance.

Is the electricity in the jet of steam developed by the simple evaporation of water in the boiler, by the expansion of high-pressure steam in the air, or by the condensation of the steam, that is in its transition from the gaseous state to that in which it begins to become visible?

It would of course be easy to decide the first question by cementing a glass tube containing a metallic wire into the boiler, the inner portion of which being of course in contact with the steam in the chamber, and precautions being taken to prevent the escape of steam either into the cylinder or the open air.

If I recollect right, during the process of evaporation, the evaporated part has been generally found to be negative electric in respect to the remaining liquid; during condensation the reverse takes place. According to Mr. Armstrong's account, the electricity of the jet was positive, and seems therefore to correspond with the electricity developed by the process of the condensation of steam. If we ascribe the electricity contained in the jet of steam simply to the evaporation of the water in the boiler, the opposite electric state of the boiler seems difficult to be explained by the laws of common electricity, because there appears to me to be no reason why the steam in contact with the inside of the iron sheets of the boiler should not discharge its electricity the

* Communicated by the Author. † See our last Number, pp. 370, 375.

same as when it comes in contact with the outside, except the inside of the sheet iron, by a process of oxidation, becomes a non-conductor of electricity in respect to the outside. An incrustation, according to Mr. Armstrong's account, was found in the boiler only as high as the water reached; but in boilers in which the water becomes very muddy, and which, therefore, are apt to prime, a sort of thin incrustation is often spread over the whole interior of the boiler as well as the safety-valve, and therefore the state of the interior of the steam-chamber ought to be very closely examined.

The number of sparks obtained by Mr. Armstrong from the boiler at a distance of a quarter of an inch, amounted to between 60 and 70 per minute. If we assume the quantity of water necessary for a 28-horse power high-pressure engine to be 2.47 cubic feet per minute, supplied by two boilers; one boiler evaporated, therefore, 1.23 cubic feet per minute, and the evaporation of 35.5 cubic inches of water with two ounces of Newcastle coal would be necessary to produce one spark of a quarter of an inch length per second, a quantity of electricity which seems to bear no proportion with the small quantity of electricity produced during simple evaporation on a small scale.

But Mr. Armstrong's experiments seem distinctly to indicate that the electricity of the steam depends chiefly on its density, and the great quantity of free electricity may, therefore, perhaps, be made sensible by the rapid expansion of high-pressure steam, and may perhaps have some relation to the quantity of free caloric becoming latent during the expansion of high-pressure steam. I scarcely need here mention the observation of Mr. Hare, that the operation of his deflagrator was entirely suspended by the operation of the common galvanic trough apparatus; besides, all conductors of electricity during mutual friction develop caloric, whilst non-conductors of electricity, on the contrary, during mutual friction, develop, instead of caloric, electricity.

It seems to me a great question whether the electricity of the steam was not in close connexion with the deposit, or the induration of the deposit upon the plates of the boiler.

I have already shown in an article on steam-boiler explosions, published in the *Mechanics' Magazine*, that those incrustations were composed of a series of distinct layers, sometimes very easily separable, and which proves that the induration or crystallization of these layers, notwithstanding the continuous evaporation and feeding, must have been occasioned at certain intervals, and that one layer must have already

been in an indurated state before the other was deposited. The layers assume a crystalline form only when they are in close contact with the iron plates.

I have also shown in the above-mentioned treatise, that during the deposition of certain salts held in solution by the boiling water the ebullition became interrupted, taking place only at intervals, and always with a sort of explosion or sudden development of steam, which often caused the glass flask to burst. During these sudden explosions the electricity of the escaping steam became so distinct, that it was readily indicated by a common gold-leaf electroscope; the electricity of the steam, on the contrary, escaping under ordinary circumstances being so feeble, that it cannot be detected without the aid of a condensator.

The development of electricity during the crystallization of certain salts is very well known, and many chemical deposits occur only under a certain pressure, to which the liquid containing them is subjected. Thus the carbonaceous deposits in common gas retorts are entirely obviated when the gas from the coals is evolved without pressure in the retorts, or even in a partial vacuum.

The columns of vapour and smoke arising from the craters of volcanos generally discharge flashes of lightning in all directions, and it is obvious that the discharged electricity is owing to the expansion or condensation of the escaping water gas, if not to a chemical separation in the column of smoke ascending from the crater with an immense force.

The electricity in thunder-clouds seems likewise to arise from condensation. I had once the good fortune to be immersed in a thunder-cloud hovering round the summit of Mount Brenner in the Tyrol, having with me at the time a barometer, thermometer, hygroscope, and an electroscope. I saw the clouds forming around me on the summit of the mountain into vaporous bodies of an irregular roundish shape, which seemed to retain their form by an attractive force arising from the centre of each individual cloud, as they had not the slightest tendency to amalgamate with each other. The hygroscope close to the cloud was not at all affected, and only when immersed in the cloud, it turned first a few degrees, indicating after a few minutes the highest degree of moisture, and sinking gradually back to its first point. This fluctuation continued as long as I had time to observe it. The electroscope was likewise not affected at all outside the cloud. Immersed in the cloud the gold leaves began gradually to separate, the barometer at the same time slightly rising, and after each discharge of lightning both instruments returned to their

original state. From these observations it would appear that with every flash of lightning the cloud became exhausted of its electricity and recharged itself for each succeeding flash.

The air in the cloud seems to move from the periphery to the centre, of the nature of a whirlwind, fluctuating with the leaves of the electroscope, and I had sufficient time to witness twenty-one electric discharges from the cloud in which I was immersed, when the wind became so violent, that the instruments were broken, and I was obliged to cling to the stump of a tree to save myself from being blown over the precipice ; but the uproar around me was increasing and fluctuating with the electric discharges from the clouds, and the rapid alternations of wet and dry in the clouds, was during the whole time in exact coincidence with the electric discharges.

LXVI. *On the Electricity of Effluent Steam.* By W. G. ARMSTRONG, Esq.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

MY letters to Professor Faraday on the remarkable development of electricity which has recently been discovered in a jet of steam issuing from a steam-engine boiler in this neighbourhood, having already appeared in your publication, it is, of course, unnecessary for me here to repeat the circumstances detailed in those letters. I shall therefore take up the narrative of my proceedings, relative to this curious subject, at the point at which the second of those letters concludes.

Having found electricity in all the three boilers I had examined in which water from the neighbouring colliery was used, and not having discovered any indications of it in the boiler which was supplied with rain-water, I was naturally led to believe that the effects I have described were attributable to the peculiar nature of the water from which the electrical steam was produced ; and, under this impression, I lost no time in visiting some other high-pressure boilers in the same district, which were also supplied with colliery water, strongly impregnated with lime and other mineral matter. The steam from the safety-valves of these boilers also proved to be electrical, but not to such an extent as I had reason to anticipate from the similarity of the circumstances to those under which electricity was developed in such an extraordinary degree at Seghill. I then proceeded to try a number of boilers in this town and neighbourhood, in which steam was

propagated under different pressures, and from water of various descriptions; and by insulating myself and holding a conducting-rod in the steam discharged from the safety-valves, I succeeded in every instance in obtaining electrical sparks, which varied in the different cases from about one-fourth to about half an inch in length.

In company with Mr. Robert Nicholson, the engineer of the Newcastle and North Shields railway, I next tried the boilers of the locomotive engines used on that railway, and finding electricity in great abundance in the ejected steam from these boilers, I determined, with Mr. Nicholson's permission and assistance, to institute a set of experiments upon one of them, with a view to a fuller investigation of the subject.

I shall now briefly describe such of these experiments as have been the most marked in their results, and shall divide them into two classes, first taking those which were chiefly intended to exhibit the extent to which electricity existed in the issuing steam, and then proceeding with the experiments which were undertaken to ascertain the cause of the electric development. Nearly all the experiments were made at night, under cover of the engine-shed, and the atmosphere was generally humid; but when it happened to be otherwise, the quantity of electricity derived from the jet was greatly increased.

Upon trying the steam in the first instance by the method adopted in the previous cases, that is to say, by standing on an insulated stool and holding with one hand a light iron rod immediately above the safety-valve, while the steam was freely escaping, and then advancing the other hand towards any conducting body, sparks of about an inch in length were obtained: but it was soon observed, that by elevating the rod in the steam the electricity was gradually increased, and that the maximum effect was not attained until the end of the rod was raised five or six feet above the valve, at which point the length of the sparks occasionally reached two inches. Small sparks were even obtained when the rod was wholly removed from the steam and held in the atmosphere at the distance of two or three feet from the jet, and the electricity thus drawn from the air was positive, like that of the steam. When the rod was extended into the cloud of vapour which accumulated in the upper part of the shed, electricity was drawn down as by a lightning-conductor from a thunder-cloud. I endeavoured to ascertain whether any precipitation of moisture, analogous to the formation of rain, accompanied

the abstraction of electricity from the steam, and a sprinkling of wet was undoubtedly felt on the face and hands by the person holding the rod, so long as he remained insulated, but the effect ceased as soon as the insulation was destroyed.

After fully trying the steam with a simple iron rod, as a conductor, recourse was had to other conductors which presented a larger surface to the steam, but the effect was not materially increased until a bunch of pointed wires of different lengths was attached to an iron rod and held in the issuing steam, with the points presented downwards. The iron rod terminated in a round knob at the end next the hand, and from this knob sparks of the *measured length of four inches* were actually drawn, almost as rapidly as they could be counted, while a stream of electricity was at the same time passing off from the rod, at the part which most nearly approached the chimney of the engine. Very perceptible sparks were also obtained when the points were held in a clear atmosphere, at the distance of at least eight feet from the nearest part of the jet.

In all the preceding experiments, the effect appeared to be proportionate to the quantity of steam discharged from the valve, when other things remained the same; and the electricity became quite imperceptible when the escape was very inconsiderable.

By abruptly raising the valve when the engine-shed was dark, the edges of the lever and margin of the brass cup which surrounded the valve, were rendered distinctly luminous with rays of positive electricity which were strongest the instant the valve was lifted, and then quickly subsided, becoming very faint after the lapse of a second.

In proceeding to investigate the cause of this extraordinary development of electricity, the first question which I proposed for inquiry was, Where does the steam first become electrical, that is to say, is it electrical in the boiler, or if not, does it become so in passing through the orifice, or not till it escapes into the air? In order to determine which of these three suppositions was correct, the apparatus represented in the annexed figure, and of which the following is a description, was employed.

A is a glass tube passing into the steam chamber through the cock B, which was screwed into a hole in the top of the boiler, and was furnished with a stuffing-box to prevent escape between the outside of the tube and inner surface of the cock; C is a stop-cock affixed to the upper end of the glass tube,

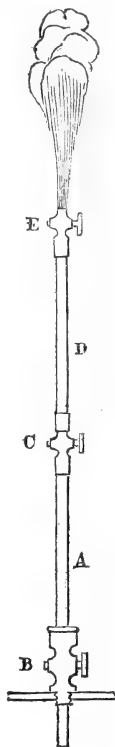
and upon which cock is screwed a second glass tube D terminating in another stop-cock E.

The application of this apparatus will be easily understood. If the steam were in the same state of electricity in the boiler as when it issued into the air, it would necessarily communicate positive electricity to the insulated cock C, in passing through the tube. Or, if the steam acquired its electricity by friction, or otherwise, in the channel through which it was discharged, it could only, in the present instance, do so *at the expense* of the cock C, which, being insulated, would in that case indicate negative electricity. Or, lastly, if the electricity were developed by condensation, expansion, or any other cause which came into operation after the steam escaped into the air, then the cock C would have neither positive nor negative electricity.

Previously to inserting the lower glass tube in the boiler, the steam was allowed to blow off through the large cock B, and the jet which issued from it proved, to the surprise of every one present, almost destitute of electricity. This result completely vitiated the inference I had drawn from the circumstance of not finding electricity in the steam from the rain-water boiler before alluded to, in which case, as I have already stated in my second letter to Professor Faraday, the jet was obtained from the gauge cock.

The lower glass tube, without the upper one attached to it, was then passed into the boiler, and a highly electrical jet was obtained from it, which communicated positive electricity to the stop-cock C, from which the steam was discharged. The upper tube was accidentally broken in screwing it on to the lower one, leaving only about three inches of glass above the cock C. Under these circumstances the cock C still continued highly charged with positive electricity, and a pale lambent light flashed at short intervals down the inside of the tube from the cock towards the boiler.

Having replaced the broken glass tube with a new one, the experiment was tried again on a subsequent evening, and the jet being now removed to a much greater distance than before from the cock C, no electricity whatever could be detected in that cock, while the one above it indicated positive



electricity in a very high degree. It therefore became pretty evident that the electricity was not developed until the steam issued into the atmosphere, and that the upper stop-cock derived its electricity from its contiguity to the jet. One circumstance alone seemed in some degree to militate against this supposition, namely, that the electricity of the cock E was greatly increased when the cock C was partially closed, as if the expansion which in that case took place in the upper tube rendered the steam electrical previously to its reaching the cock from which the jet was discharged. No negative electricity, however, could be discerned in any part of the apparatus, and without a development of negative electricity, I cannot see how positive electricity can possibly arise from expansion. The more probable explanation of the effect appeared to be, that the partial closing of the middle cock shortened the transparent or non-conducting part of the jet, and thereby caused the electricity to be more readily communicated from the opaque part of the jet.

In consequence, no doubt, of increased accumulation of electricity which was thus occasioned in the highest cock, together with the unavoidable dampness of the surrounding medium, the upper glass tube, and the cock above it, became illuminated in the most singular and beautiful manner. Flashes of wavering light flickered round the exterior surface of the glass, and darted from it to the distance of three or four inches, while strong rays of electrical light streamed from the angular parts of the cock, and the flashes from the glass were accompanied by a snapping noise which was distinctly audible amidst the hissing of the steam when the ear was advanced within a short distance from the tube.

The upper glass tube was then removed, and as an additional test of the non-existence of free electricity in the interior of the boiler, a pointed wire was thrust down through the cock C and tube A into the steam, and effectual means were used to prevent the escape which would otherwise take place at the cock C, in consequence of the tap remaining open to admit the wire. Now this wire being insulated by the glass tube and communicating with the insulated cock C, must have rendered that cock electrical, if the steam were electrical in the boiler; but not the slightest indication of electricity could, under these circumstances, be found in the cock.

Having withdrawn the pointed wire from the tube, another glass tube, of which the sectional area was about ten times greater than that of the one inserted in the boiler, was then attached to the cock C, in the same manner as the tube D had been before. The comparatively large bore of this tube

allowed the steam to expand in a very great degree before it issued into the air, and caused it to be discharged in the state of low-pressure steam; but no diminution of electricity could be perceived in the jet, when thus attenuated; so that the electrical development does not appear to depend upon the degree of violence with which the steam comes in contact with the atmosphere.

The entire absence of negative electricity seemed to preclude the possibility of the phenomena arising from expansion, and the only remaining supposition appeared to be, that the condensation which took place in the jet, set free the electricity which the steam had absorbed in the process of evaporation. This supposition had been previously rendered probable, when it was discovered that the upper and most opaque part of the jet yielded the most electricity, although I was at first inclined to attribute that circumstance to the dampness of the steam, in that part of the jet, rendering it a better conductor, and causing it to part more readily with its electricity. Experiments were next, therefore, commenced to ascertain the effect of insulating the boiler, and wholly condensing the steam; but these require repetition before they can be much relied upon. The great difficulty is to effect insulation amidst so much moisture, but I have no doubt that with a little perseverance this object will be accomplished, and I trust I shall be able to furnish, in time for insertion in the next Number of the *Philosophical Magazine*, such further results as will set the question at rest.

I am, yours, &c.

Newcastle upon-Tyne, Nov. 18, 1840. WM. GEO. ARMSTRONG.

LXVII. *Further Experiments on the Electricity of Steam.*

By H. L. PATTINSON, Esq., F.G.S.

To the Editors of the Philosophical Magazine and Journal.

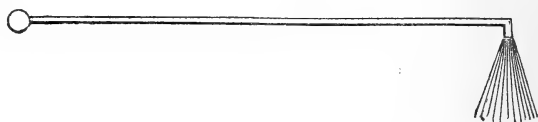
GENTLEMEN,

SINCE my last letter to you, dated the 19th ult. (published at p. 375 of this volume), relative to the electricity of steam issuing from two boilers at Cramlington Colliery, the subject has been further pursued both by myself and others, and sparks have been obtained from the steam of various boilers, in every direction. The mode of operating has generally been that described in my letter, viz. suffering the steam to escape from the safety-valve of the boiler tried, and testing its electricity by holding in it a shovel or an iron rod, the operator standing upon an insulating stool. Sometimes the indications have been very slight, and sometimes there

has hardly been any appearance of electricity in the steam; but in such cases the trials have been generally made under unfavourable circumstances, and from all that has yet been done, the presumption is certainly that steam is always more or less electrical. It is not, however, always electrical to the same extent under the same pressure, as I shall presently show.

Mr. Armstrong was the first to experiment with a locomotive engine-boiler (one used on the Newcastle and North Shields railway), from which he obtained very striking results. The directors of the Newcastle and Carlisle railway, through their secretary, Mr. Adamson, gave me permission to experiment upon the boilers of the locomotive engines on that line, and I now beg to lay before you the results I have obtained. In preparing for, and performing these experiments, I have, as before, been assisted and accompanied by Mr. Henry Smith, and I have received the most willing and efficient aid from Mr. Anthony Hall of Blagdon, the mechanical engineer on the railway.

1. A copper rod, half an inch in diameter, and five feet long, was provided, made hollow for lightness; this was terminated at one end by a two-inch ball, and at the other (which was bent at a right angle) by ten or twelve sharp-pointed wires, spread out in every direction to collect the electricity more perfectly from the steam.



2. The Wellington locomotive engine, immediately after coming to the station with passengers, was first tried. At this time the steam was blowing forcibly out of the safety-valve, at a pressure of fifty-two pounds per inch. On holding the pointed conductor in this current of steam, with its points downwards, the individual holding it standing at the time on an insulating stool, sparks three to four inches long were given off from his person to the boiler. The sparks were largest when the valve was held down a minute or two and then suddenly lifted, so as to suffer a large volume of steam to escape with great rapidity. By this management the sparks were frequently four inches long, and occasioned considerable pain to the person on the stool, even when given from a brass ball held in his hand. The sparks were largest when the points of the conductor were held in the steam about two feet above the valve; but larger sparks were ob-

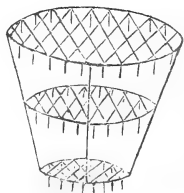
tained when it was held much higher; and indeed sparks were obtained by holding the conductor entirely out of the cloud of steam, and at a distance from it, for the air in the wooden shed in which we operated became speedily electrical throughout. The electricity was positive.

3. The steam in the boiler was now gradually run down to see how the electrical condition would vary with the pressure. At forty pounds per inch the sparks became much less, the largest not reaching three inches. At thirty pounds the largest spark did not reach two inches; at twenty pounds it became barely an inch; at ten pounds not more than from one-fourth to one-half of an inch; and at five pounds per inch pressure the spark was hardly perceptible. But if at any pressure the valve was held down a few minutes so as to suffer the steam to accumulate and then suddenly opened, there was always a great increase, for an instant, of the electrical effects.

4. Another boiler, that of the *Lightning* engine, which had also just come in from a trip, and had its steam blowing off forcibly, at a pressure of fifty pounds per inch, was now tried in exactly the same way as the *Wellington*. On holding the pointed conductor in the steam, whether regularly blowing off at the valve or escaping with great rapidity from the sudden lifting of the valve, it did not yield a spark more than one-fourth of an inch long. We then blew a quantity of water out of the boiler of the *Lightning* until it barely covered the tubes inside, and on afterwards testing its steam blowing off at fifty pounds per inch, the spark was found increased to nearly two inches in length. The steam of the *Lightning* was, however, much less electrical than the steam of the *Wellington* at the same pressure, under all the circumstances of our experiments.

5. The strong current of steam and water issuing from the boiler of the *Lightning* when the water was blown out of it as just stated, was tested for electricity, but no indications could be perceived whatever.

6. A very large conductor had been provided, made of zinc two-inch tubing, in this way,—three rings were made of this tubing, respectively three feet, two feet, and one foot diameter. These rings were attached to each other a foot and a half apart by side pieces, so as to form a hollow frustum of a cone, three feet high, with ends three feet and one foot diameter respectively. The inside of this cone was laced across with copper wire, and the whole bristled with pointed wires in every direction. By means of a long



iron bar, placed upright in a cask of rosin (both to insulate it and to serve as a foot), and a horizontal arm projecting from it, made to slide up and down on the vertical bar, the large conductor could be placed in any part of the cloud of steam issuing from the valve, and the electricity given off could be conveyed from it in any direction. Care was taken to round off all parts of this conductor, so as to avoid sharp points and angles as much as possible. On trying this large conductor in the current of steam from the Wellington, we were disappointed to find that it did not yield a longer spark than the small pointed copper rod with which we had previously experimented. The spark was larger in volume, but it did not possess greater intensity. It never struck through more than three inches of space, but its effect upon the person when taken was very violent and painful. Our intention was to have ascertained the rate at which large jars could be charged from the steam, in order to form some idea of the quantity of electricity given off; but the evening had become very damp, and the air was so moist, that we could not procure sufficient insulation, and were obliged to relinquish the attempt.

7. When the large conductor was held in the cloud of steam with its lower part or apex about two feet above the valve, it gave off numerous and powerful sparks; but if at this time the points of the small conductor were placed by a person connected with the ground in the steam below the large conductor a foot above the valve, the electricity given off by the large conductor was very materially diminished.

8. By means of screws, the entire engine (the Wellington) was raised off the rails and placed upon blocks of baked wood, so as to insulate it entirely. The steam being now blown off at the valve, the boiler and engine became strongly electrical with negative electricity; points placed upon any part of the engine exhibiting the peculiar star of the negative element, and threads suspended from the engine being repelled by excited sealing-wax. The steam was at the same time strongly positive, and when a point connected with the conductor held in the steam was brought near a point attached to the insulated boiler, the pencil upon the former and star upon the latter were beautifully decisive as to the electrical states of each.

9. I repeated Volta's experiment by placing a hot cinder upon the cap of a gold-leaf electrometer, and projecting a few drops of water upon it, when the leaves diverged strongly with negative electricity. I observed, that when the cinder was very hot, and the production of the steam consequently very rapid, the electricity given out was always most powerful.

10. I then insulated an iron pan, twelve inches diameter and two inches deep, and attached to it a pith-ball electrometer, with balls three-eighths of an inch diameter, and threads five inches long, and also attached to the pan a metallic wire, the pointed extremity of which was placed about one-twentieth of an inch distant from the point of another wire connected with the ground. The iron pan was then filled with cinders, very hot, from a wind-furnace, and on projecting upon them a few ounces of water, steam was evolved with great rapidity, and at the same moment the pith balls diverged to the distance of an inch, and sparks passed between the metallic wires. This was several times repeated.

These experiments enable us, I conceive, to give a clear explanation of the electrical phenomena presented by steam. There is no doubt whatever, as Dr. Faraday conjectures in his note to Mr. Armstrong's paper in your last Number, "that this evolution of electricity by vaporization is the same as that already known to philosophers on a much smaller scale." The electricity appears to originate at the instant of vaporization, and the steam as it collects within the boiler is electrified with positive electricity, the water and metallic boiler being at the same time negative. In this condition the electricity of both is latent, like the electricity of the two plates of an excited electrophorus; but the instant steam is suffered to escape, its positive electricity, being carried off along with it, and out of the influence of the equivalent quantity of negative electricity in the boiler, becomes free, and hence the steam is electrical with positive electricity. The same thing takes place with the boiler, in which negative electricity is set at liberty as the steam escapes, and which becomes evident on insulating the boiler.

When steam much mixed with water, or what engine-men call "wet steam," escapes from a boiler, it evidently cannot be very highly electrical, for the negative water will tend to neutralize the positive steam, and this may perhaps in some measure account for the increased effect in the Lightning on lowering the water within its boiler, and for the increase of intensity in every boiler, observed when the valve has been forcibly held down and is suddenly opened; but it does not seem sufficient to account *entirely* for these variations of intensity, nor for the difference of intensity in different boilers at the same pressure. It is therefore probable that chemical action between the metal of the boiler and the water has something to do with exalting the electrical condition of the steam at the moment it is generated; but this part of the subject certainly requires further investigation. By far the most powerful ef-

fects up to this time have been obtained from locomotive engines, in which water is heated in contact with brass tubes. How far this may influence the production of electricity, further experiments must determine. It is certainly somewhat curious to consider the splendid locomotive engines we see daily in the light of enormous electrical machines; but this they undoubtedly are; the steam is analogous to the glass plate of an ordinary machine, the boiler to the rubbers; and a conductor properly exposed to the escaping steam gives out torrents of electricity.

I am, Gentlemen,

Your obedient Servant,

Bentham-Grove, Gateshead,
November 21, 1840.

H. L. PATTINSON.

LXVIII. *On the Motion of a small Sphere vibrating in a resisting Medium.* By the Rev. J. CHALLIS, Plumian Professor of Astronomy in the University of Cambridge*.

IN the London and Edinburgh Philosophical Magazine for September, 1833 (vol. iii. p. 186.), I have given a solution of the problem of the resistance to the motion of a ball-pendulum vibrating in the air, by making use of the principle of the conservation of *vis viva*, and assuming that for slow vibrations the motion of the air surrounding the ball is the same as if the fluid were incompressible. I have given another solution in the Cambridge Philosophical Transactions (vol. v. part ii. p. 200.), by adopting the above assumption without using the principle of the conservation of *vis viva*; and in the latter solution it is not taken for granted, as in the other, that the same considerations apply to fluid motion directed to or from a moving centre, as to motion to or from a fixed centre. The two methods lead to the same result. In 1835, M. Plana published at Turin a Memoir (for a copy of which I am indebted to the kindness of the author) containing a solution of the problem in question, the same in principle as that of Poisson in vol. xi. of the *Mémoires* of the Paris Academy of Sciences†, with the difference of treating separately the motions in a compressible and an incompressible fluid, and so obviating some objections to which Poisson's reasoning appeared liable. M. Plana adverts to my communication in the Philosophical Magazine, and subjoins a translation of it, but is unwilling to admit the correctness of the principle of the method I have employed, apparently for no other reason than that it leads to a result differing from his own.

* Communicated by the Author.

† Poisson's memoir is also inserted in the *Connaissance des Temps* for 1834.

The two methods are, in fact, so dissimilar in principle, and in their results, that if one is right the other must be wrong. But after the lapse of some years I am not able to discover any error either in the principle or the details of the method I have employed in the Philosophical Magazine, nor of that in the Cambridge Philosophical Transactions. The object of my present communication is to give a *third* solution, which applies expressly to vibrations of the ball in a *compressible* fluid.

It will be proper to begin with proving generally that the same equations apply to the motion of the fluid when directed to or from a moving centre, as when directed to or from a fixed centre.

Considering, first, the motion of the fluid to be in the direction of radii from a fixed centre, conceive two spherical surfaces described about this centre at the distances r and r' differing very little from each other; and let the interior one pass through the point at which we consider the motion. Conceive also a conical surface, having its vertex at the centre of the spherical surfaces and its vertical angle indefinitely small, to intersect with its axis the interior spherical surface at that point. Let m^2 = the small portion of the interior spherical surface included by the conical surface; then $\frac{m^2 r'^2}{r^2}$ = the corresponding portion of the outer surface. It

will be assumed that during a very small time δt , the velocity and density of the fluid which passes the area m^2 are uniformly v and ρ ; and, similarly, that the velocity and density of the fluid passing in the same time the corresponding area of the other surface are uniformly v' and ρ' . Then the quantity of fluid which passes m^2 in the time δt is $m^2 \rho v \delta t$; and that which passes the other area in the same time, $\frac{m^2 r'^2}{r^2} \cdot \rho' v' \delta t$.

The increment of matter in the included space is, therefore, $-m^2 \delta t \left(\frac{r'^2 \rho' v'}{r^2} - \rho v \right)$, the velocities v and v' being reckoned positive when directed *from* the centre. The space itself is ultimately $m^2 (r' - r)$. Hence the increment of density $\delta \rho$ is equal to $-\frac{m^2 \delta t (r'^2 \rho' v' - r^2 \rho v)}{m^2 (r' - r)}$.

Consequently,

$$\frac{\delta \rho}{\delta t} + \frac{r'^2 \rho' v' - r^2 \rho v}{r^2 (r' - r)} = 0;$$

464 Rev. J. Challis on the Motion of a small Sphere
and passing from differences to differentials,

$$\frac{d\rho}{dt} + \frac{d \cdot r^2 \rho v}{r^2 dr} = 0; \quad . \quad . \quad . \quad . \quad (1.)$$

where, from the nature of the investigation, the differential coefficients are evidently partial.

Now suppose the motion of the fluid to be directed to or from a moving centre, and let two spherical surfaces separated by a very small interval be described about this centre, the interior one always passing through the point of space at which we consider the motion. On account, therefore, of the motion of the centre, the spherical surfaces will not be stationary. We may, however, conceive a conical surface, described as in the former case, to have its axis always passing through the moving centre and the point of space at which the motion is considered, and to include a given small portion m^2 of the interior spherical surface. The velocity and density of the fluid passing the area m^2 may, as before, be considered uniform during a very small time δt ; as may also, without entailing error, the velocity and density of the fluid passing the portion of the outer surface always included by the conical surface. Hence, using the same letters as in the case of a fixed centre, the quantity of fluid which passes m^2 in the time δt is $m^2 \rho v \delta t$. We have now to ascertain the quantity of fluid which in the same time passes the corresponding area of the exterior surface. Let r and r' be the radii of the two concentric surfaces at the beginning of the interval δt , and let α be the velocity of the centre resolved in the direction of r . Then after an interval τ , less than δt , the radii of the surfaces are $r \pm \alpha \tau$ and $r' \pm \alpha \tau$ ultimately. Hence the area

$$\begin{aligned} &\text{of the outer surface corresponding to } m^2 = m^2 \cdot \left(\frac{r \pm \alpha \tau}{r' \pm \alpha \tau} \right)^2 \\ &= \frac{m^2 r'^2}{r^2} \cdot \left(\frac{1 \pm \frac{\alpha \tau}{r'}}{1 \pm \frac{\alpha \tau}{r}} \right)^2 = \frac{m^2 r'^2}{r^2}, \text{ by neglecting terms that} \end{aligned}$$

may be neglected, since by hypothesis r' differs very little from r , and $\alpha \tau$ is very small. This result is independent of τ , and is the same as if the centre had been fixed. The rest of the reasoning would consequently conduct to the equation (1.). Hence from this equation combined with the known

equations p (the pressure) $= a^2 \rho$, and $\frac{dp}{\rho dr} + \left(\frac{dv}{dt} \right) = 0$,
equations applicable to motion directed to or from either a

fixed or a moving centre may be deduced. I will not stop to make the deduction, which presents no difficulty, but at once employ the equations given in the Treatises on Hydrodynamics for motion propagated from a fixed centre. (Propagation towards the centre is excluded by the nature of the question.) These equations are (putting $1+s$ for ρ),

$$v = \frac{f'(r-at)}{r} - \frac{f(r-at)}{r^2} \quad (2.), \text{ and } as = \frac{f'(r-at)}{r} \quad (3.), \text{ which}$$

as they contain arbitrary functions, apply immediately to the arbitrary disturbance given to the fluid. In the problem before us they apply, therefore, to the motion given to the fluid by the vibrating sphere at its surface. For as the sphere is supposed to be perfectly smooth and consequently to impress motion only in a direction normal to its surface, the motion at the surface is plainly directed to or from a moving centre. The arbitrary condition of the motion is that at a given distance (r), equal to the radius of the sphere from the centre regarded as fixed, and at a given point of the surface of the sphere, the velocity impressed follows either exactly or very approximately the law of a vibrating pendulum. Let the velocity of the centre of the sphere at any time t be $V \sin bt$. Then for any point the radius to which makes an angle θ with the direction of the motion, we shall have the normal velocity v equal to $V \cos \theta \sin bt$. Hence, putting for brevity $u = f(r-at)$, and substituting in the equation (2.), it will be

$$\text{found that } \frac{du}{dt} + \frac{a}{r} u + V a r \cos \theta \sin bt = 0,$$

an equation in which u and t are the only variables, and which is true whatever be t . The integral of this equation is

$$u = C e^{-\frac{at}{r}} - V r^2 \cos \theta \cos \phi \sin (bt - \phi),$$

$\tan \phi$ being put for $\frac{br}{a}$. The term involving C will be insensible for all but very small values of t , on account of the factor $e^{-\frac{at}{r}}$, and may therefore be omitted. Hence, by differentiating and putting $a \tan \phi$ for br ,

$$\frac{du}{dt} = -V a r \cos \theta \sin \phi \cos (bt - \phi).$$

Now the pressure at the point of the sphere we are considering is equal to $a^2 s$, or by equation (3.) $a \cdot \frac{f'(r-at)}{r}$, or

$$- \frac{1}{r} \cdot \frac{du}{dt}. \text{ Hence this pressure is } V a \cos \theta \sin \phi \cos (bt - \phi).$$

And by integrating in the usual way to obtain the pressure on the whole sphere, it will be found to be $\frac{4\pi V a r^2}{3} \cdot \sin \phi \cos (b t - \phi)$. This is reckoned positive in the direction contrary to that of the motion of the sphere. Hence if σ = the ratio of the specific gravity of the fluid to that of the sphere, the accelerative force of the resistance in the positive direction of the motion is $-\frac{V a \sigma}{r} \cdot \sin \phi \cos (b t - \phi)$. If λ = the distance to which motion is propagated in the fluid in the time of one vibration of the sphere, $b = \frac{2\pi a}{\lambda}$, and consequently, $\tan \phi = \frac{2\pi r}{\lambda}$. This is an exceedingly small quantity. Hence very approximately $\sin \phi = \frac{2\pi r}{\lambda} = \frac{b r}{a}$, and the accelerative force of resistance = $-V b \sigma \cos b t$. Again, if x = the distance of the centre of the sphere at the time t from the mean place about which it is oscillating, $\frac{d x}{d t} = V \sin b t$, and $\frac{d^2 x}{d t^2} = V b \cos b t$. Hence the accelerative force of the resistance = $-\sigma \cdot \frac{d^2 x}{d t^2}$. The length of the pendulum being l and the force of gravity g , the accelerative force of gravity, taking account of the buoyancy of the fluid, is $-\frac{g x}{l} (1 - \sigma)$. Hence,

$$\frac{d^2 x}{d t^2} = -\frac{g x}{l} (1 - \sigma) - \sigma \cdot \frac{d^2 x}{d t^2},$$

and consequently

$$\frac{d^2 x}{d t^2} = -\frac{g x}{l} \cdot \left(\frac{1 - \sigma}{1 + \sigma} \right).$$

This is the result I obtained by my two former methods. As it does not contain a , it is applicable to any resisting medium, supposing the vibrations to be slow. Putting the factor in brackets, under the form $1 - n\sigma$, we shall have $n = \frac{2}{1 + \sigma}$. For a brass ball of specific gravity 8, vibrating in air, $n = 2$ very nearly; and for the same vibrating in water,

$n = 1.78$. The experiments of Bessel give for these two cases, 1.95 and 1.63.

I do not consider the above solution of value for the numerical results to which it leads, so much as because it serves to establish the principles to be adopted in the treatment of another problem (perhaps the most important that could be proposed in the present state of physical science), the solution of which has hitherto been unattempted, viz. *if a minute spherical atom were subject to the mechanical action of the vibrations of a very elastic medium, like those which take place in air, would it, in addition to a vibratory motion, receive also a permanent motion of translation?* I propose at a future opportunity to state my reasons for considering this an important question, and to advance some ideas respecting the method in which I conceive it may be answered.

Cambridge Observatory, Nov. 16, 1840.

LXIX. *On the Heat of Vapours and on Astronomical Refractions.* By JOHN WILLIAM LUBBOCK, Esq., Treas. R.S., F.R.A.S. and F.L.S., Vice-Chancellor of the University of London, &c.

[Continued from p. 280.]

On the Conditions of the Atmosphere, and on the Calculation of Heights by the Barometer. (Resumed.)

AS the expression which has served to calculate the temperatures evidently represents the state of the atmosphere far within the limits of the applicability of this or any other formula founded upon a state of repose to an atmosphere continually agitated by currents, it must of course serve to eliminate the density and to obtain an expression for the height in terms of the pressures and temperatures at the extremities of any atmospheric column.

If z be the altitude of the place above any fixed point, a the distance of the fixed point from the centre of the earth, g the force of gravity,

$$\frac{dp}{g} = - \frac{g a^2}{(a + z)^2} dz,$$

and putting the expression for g' , at vol. xvi. p. 440,

$$\frac{k(1 + \alpha \theta)(p^\beta - E) dp'}{p'(p'^\beta - E)} = - \frac{g a^2 dz'}{(a + z')^2}.$$

2 H 2

This expression can be integrated, and I find, supposing $z = 0$, after a proper determination of the constants,

$$\frac{z'}{1 + \frac{z'}{a}} =$$

$$\frac{k(1 + \alpha\theta)}{g} \frac{\{p^\beta - p'^\beta\} \left\{ \frac{1}{\alpha} + \theta' \right\}}{\beta \left\{ p'^\beta \left(\frac{1}{\alpha} + \theta' \right) - p^\beta \left(\frac{1}{\alpha} + \theta \right) \right\}} \text{Nap. log} \left\{ \frac{\left(\frac{1}{\alpha} + \theta' \right)}{\left(\frac{1}{\alpha} + \theta \right)} \left(\frac{p'}{p} \right)^\beta \right\}$$

If the variation of the force of gravity be neglected, the pressures p, p' may be represented by the heights of the barometer h, h' . If M be the *modulus* or the quantity by which Naperian logarithms must be multiplied to give common logarithms, Laplace makes

$$\frac{k}{gM} = 18337^m.46. \quad \log M = 9.6377843.$$

In order to give an example of the use of this expression, I take the 21st observation of Gay Lussac,

$$h = .76568$$

$$\theta = 30.75$$

$$h' = .3339$$

$$\theta' = -7.00$$

$$\log 18337.46$$

$$= 4.2633392$$

$$\log (1 + \alpha\theta)$$

$$= 0.0474015$$

$$\log \left\{ \frac{(h^\beta - h'^\beta) \left(\frac{1}{\alpha} + \theta' \right)}{h'^\beta \left(\frac{1}{\alpha} + \theta' \right) - h^\beta \left(\frac{1}{\alpha} + \theta \right)} \right\}$$

$$= 0.2696699$$

$$\log \left\{ \log \left\{ \frac{\left(\frac{1}{\alpha} + \theta' \right)}{\left(\frac{1}{\alpha} + \theta \right)} \left(\frac{h'}{h} \right)^\beta \right\} \right\}$$

$$= 8.7762776$$

$$\log \beta$$

$$3.3566882$$

$$9.5176049$$

$$3.8390833$$

$$\frac{z'}{1 + \frac{z'}{a}} = 6903.7$$

$$\log a = 6.8041168 \text{ in metres.}$$

$$z' = 6921.7 \text{ metres.}$$

If

$$\left(\frac{p'}{p}\right)^{\frac{\gamma-1}{\gamma}} = 1 - q \quad \frac{E p^{\frac{1-\gamma}{\gamma}}}{1 - E p^{\frac{\gamma-1}{\gamma}}} = -H \text{ as before, vol. xvi. p. 440.}$$

The expression for z' may be put into the form

$$\frac{z'}{1 + \frac{z'}{a}} = -\frac{k(1 + \alpha \theta)}{g H \beta} \text{Nap. log } (1 - Hq).$$

If $\gamma = 1.49138$ when $p' = 0$, $q = 1$, we get for the superior limit of the atmosphere an altitude of about 24 miles, or 38918 metres.

Ultimately the intensity of the cold deprives the air of its elasticity*. The density therefore requires in strictness to be represented by a discontinuous function; for the formula suggested in this treatise is of course only applicable so long as the air exists in the state of an elastic vapour. The freezing point of air is unknown, and we cannot decide when this condition ceases to obtain.

Delambre estimates the height of the atmosphere as deduced from the phenomena of twilight† at 70,800 metres; but this calculation is open to objection. See *Conn. des Temps*, 1841, p. 58.

I have given the example of the calculation of a height by an observation of the barometer, in order to show how my formula for the density may be employed; but however inaccurate in principle the method in use may be, it is sufficiently exact for elevations accessible to man. In all inquiries, however, connected with the condition of the higher regions of the atmosphere, and in the various hypotheses which may be made respecting the decrement of temperature, the corresponding height must be calculated by an appropriate formula, procured agreeably to the hypothesis which may be adopted. Our information respecting the state of the higher regions of the atmosphere is I think more likely to be improved by observations made in aëronautic ascents than by those made on the sides of mountains.

Let $u = -\text{Nap. log } (1 - Hq)$

$$i = \frac{k(1 + \alpha \theta)}{a g H \beta}$$

$$\frac{z'}{1 + \frac{z'}{a}} = a i u.$$

* See Poisson, *Théorie de la Chaleur*, p. 460. "On peut se représenter une colonne atmosphérique qui s'appuie sur la mer, par exemple, comme un fluide élastique terminé par deux liquides, dont l'un a une densité et une température ordinaires, et l'autre une température et une densité excessivement faibles." See also Biot, *Conn. des Temps*, 1841.

† See Delambre's *Astronomie*, vol. i. p. 337, and Lalande's *Ast.*, vol. ii. art. 2270

At the summit of the atmosphere $q = 1$, if u'' be the corresponding value of u ,

$$u'' = -\text{Nap. log } (1 - H) \quad c^{-u''} = 1 - H,$$

c being the number of which the hyperbolic logarithm is unity.

$$\frac{E p^{-\beta}}{1 - E p^{-\beta}} = -H. \quad \text{See vol. xvi. p. 440.}$$

p being the pressure at the lower station; the pressure for .76568^m or 30.14 inches of mercury in the barometer being unity.

I get, when

$$\gamma = 1.5 \quad \beta = -\frac{1}{3} \quad E = -1.192,$$

the following formula for calculating heights by observations of the barometer:

$$\frac{z'}{1 + \frac{z'}{a}} = [4.7404605] \frac{(1 + \alpha \theta)}{H} \log (1 - Hq) \text{ in French metres,}$$

$$= [5.2564585] \frac{(1 + \alpha \theta)}{H} \log (1 - Hq) \text{ in English feet,}$$

$$= [1.5338195] \frac{(1 + \alpha \theta)}{H} \log (1 - Hq) \text{ in English miles,}$$

the temperature θ at the lower station being reckoned from the freezing point.

$\text{Log } \alpha = 7.3187588$ for Fahrenheit's scale.

If we assume the 21st observation of Gay Lussac, and suppose $\gamma = 1.4$, I find

$$\beta = -.2857 \quad E = -.8405 \quad \log H = 9.6596173.$$

In Fahrenheit's scale

$$\tau = \frac{[2.9935785]}{p^\beta + .8405} - 448^\circ.$$

$$\text{Height in miles} = [1.9885722] \log (1 - Hq).$$

If we suppose $\gamma = 1.5$, I find

$$\beta = -.3333 \quad E = -1.1920 \quad \log H = 9.7354232$$

$$\tau = \frac{[30.694832]}{p^\beta + 1.1920} - 448^\circ.$$

$$\text{Height in miles} = [1.8457978] \log (1 - Hq).$$

If we suppose $\gamma = 1.6$, I find

$$\beta = -.375 \quad E = -1.5112 \quad \log H = 9.7794573$$

$$\tau = \frac{[3 \cdot 1285240]}{p^\beta + 1 \cdot 5112} - 448^\circ.$$

$$\text{Height in miles} = [1 \cdot 7506111] \log(1 - Hq).$$

Mr. Russell has calculated for me the following table in order to show in what manner the density and temperature of the atmosphere vary in the higher regions under these three different suppositions.

Height in miles.	$\beta = -\cdot 2857.$			$\beta = -\frac{1}{3}.$			$\beta = -\cdot 375.$			Height in miles.
	$p.$	$\tau.$	$\xi.$	$p.$	$\tau.$	$\xi.$	$p.$	$\tau.$	$\xi.$	
0	1·0000	+ 87	1·0000	1·0000	+ 87	1·0000	1·0000	+ 87	1·0000	0
4	·4628	+ 24	·5248	·4630	+ 24	·5248	·4631	+ 25	·5249	4
8	·1906	- 45	·2534	·1902	- 48	·2543	·1900	- 50	·2553	8
12	·0656	- 122	·1076	·0645	- 130	·1086	·0635	- 137	·1093	12
16	·0167	- 206	·0367	·0153	- 223	·0365	·0141	- 240	·0362	16
20	·0022	- 298	·0080	·0015	- 330	·0068	·0009	- 361	·0055	20
24	·0000	- 399	·0003							24
	Limit 25·81 miles.			23·896 miles.			22·52 miles.			

By making $\gamma = 1 \cdot 5$, the expression for the density becomes simplified, $\frac{1}{1 - \gamma} = -2$,

$$\rho' = \frac{\rho}{H^2} c^{-u} \left\{ c^{-u} - 1 + H \right\}^2. \text{ See vol. xvi. p. 440.}$$

$$\text{If } \frac{\rho'}{\rho} = 1 - \omega$$

$$1 - \omega = \frac{1}{H^2} \left\{ c^{-3u} - 2(1 - H)c^{-2u} + (1 - H)^2 c^{-u} \right\}$$

It must be recollected that the difficulty of determining the densities at different altitudes, and that of determining altitudes by observations of the barometer, rest in finding the accurate law of the temperature. So that if the expression which I have here suggested for the temperature be adopted, the expression for the density, and those for finding the elevation by observations of the barometer, follow as a matter of course, and their accuracy is unquestionable.

The employment of the formula in p. 467, for calculating heights, amounts to determining the constant E from the observations themselves, and not from previous observations. But if the constants are supposed to be known, as in calculating a series of observations made under the same circumstances, it is more simple to employ the expression

$$\frac{z'}{1 + \frac{z'}{a}} = a i u.$$

The day on which M. Gay Lussac made his ascent was very warm, and the values of γ and H determined from his observations may differ slightly from those mean values which will be obtained hereafter from more complete data. The preceding theory supposes implicitly that a given temperature at the earth's surface always corresponds in any given place to a given pressure; this, owing to the currents, the winds, and to other causes, is not the case; for the atmosphere is never in a state of repose, and its temperature and density are in a continual state of oscillation about their mean values. The constants γ and E may also be subject to variations from fluctuations in the quantity of aqueous vapour diffused through the atmosphere.

If the decrements of temperature are the same for equal increments of altitude, which observation shows is nearly the case at small elevations,

$$\theta - \theta' = A z',$$

θ being the temperature at the lower station, θ' at the upper, and z' as before, the altitude of the latter reckoned from the former,

$$1 + \alpha \theta' = 1 + \alpha (\theta - A z'),$$

and if the variation of the force of gravity be neglected

$$\begin{aligned} \frac{d p'}{p'} &= - \frac{g d z'}{k \{1 + \alpha (\theta - A z')\}} \\ z' &= \frac{(1 + \alpha \theta)}{\alpha A} \left\{ 1 - \left(\frac{p'}{p} \right)^{\frac{k \alpha A}{\varepsilon}} \right\} \\ g' &= g \left\{ \frac{1 + \alpha (\theta - A z')}{1 + \alpha \theta} \right\}^{\frac{g}{k \alpha A} - 1} \end{aligned}$$

p' being the pressure at the upper station, and p at the lower.

Mr. Ivory assumes, *Phil. Trans.*, 1838, p. 192,

$$\begin{aligned} \frac{1 + \alpha \theta'}{1 + \alpha \theta} &= 1 - f \log \frac{g}{g'} + (f - f') \frac{\left(\log \frac{g}{g'} \right)^2}{1 \cdot 2} \\ &\quad - (f - 2f' + f'') \frac{\left(\log \frac{g}{g'} \right)^3}{1 \cdot 2 \cdot 3} + \&c. \end{aligned}$$

But Mr. Ivory afterwards neglects the terms depending upon f' , f'' , &c., so that he virtually assumes

$$\begin{aligned}\frac{1 + \alpha \theta'}{1 + \alpha \theta} &= 1 - f \left\{ \log \frac{\varrho}{\varrho'} - \frac{\left(\log \frac{\varrho}{\varrho'} \right)^2}{1 \cdot 2} + \&c. \right\} \\ &= 1 - f \left\{ 1 - \frac{\varrho'}{\varrho} \right\} = \frac{p' \varrho}{p \varrho'} \\ \frac{p'}{p} &= (1 - f) \frac{\varrho'}{\varrho} + f \frac{\varrho'^{1/2}}{\varrho^2}\end{aligned}$$

Mr. Ivory makes the constant $f = \frac{2}{9}$, p. 197, so that

$$\begin{aligned}\frac{p'}{p} &= [9.8908555] \frac{\varrho'}{\varrho} + [9.3467875] \frac{\varrho'^{1/2}}{\varrho^2} \\ d p' &= p (1 - f) \frac{d \varrho'}{\varrho} + 2 p f \varrho' \frac{d \varrho'}{\varrho^2} \\ &= - \frac{g \varrho' d z'}{\left(1 + \frac{z'}{a} \right)^2} \\ \frac{z'}{1 + \frac{z'}{a}} &= a i u^* = \frac{p (1 - f)}{g \varrho} \log \frac{\varrho}{\varrho'} + \frac{2 p f}{g \varrho} \left(1 - \frac{\varrho'}{\varrho} \right) \\ &= \frac{k (1 + \alpha \theta) (1 - f)}{g} \log \frac{\varrho}{\varrho'} + \frac{2 k (1 + \alpha \theta) f}{g} \left(1 - \frac{\varrho'}{\varrho} \right) \\ &= [0.9635418] \log \frac{\varrho}{\varrho'} + [0.3582881] \left(1 - \frac{\varrho'}{\varrho} \right)\end{aligned}$$

for 50° Fahr. at the lower station.

As we cannot make direct observations of the temperature and density of the highest regions of the atmosphere, it becomes very important to avail of all indirect means of investigation. The problem of Astronomical Refractions furnishes us with valuable data in this respect, and any hypothesis relative to the state of the atmosphere which will not satisfy the known phænomena of refraction, must of course be discarded. In any investigation of this kind it is indispensable to employ a formula for z in terms of the density consistent with the hypothesis, which may be made respecting the decrement of temperature; it is equally indispensable to carry the integral which affords the amount of refraction through limits which are in conformity with the same supposition.

* $a i u = \sigma$ in Mr. Ivory's notation. In this page p is the pressure and ϱ is the density at the earth's surface.

[To be continued.]

LXX. *Proceedings of Learned Societies.*

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Twenty-Seventh Annual Report of the Council.

THE subject which naturally first presents itself to the Council in the preparation of their Report, is the irreparable loss the Society has sustained by the death of its illustrious and venerable President (Mr. Davies Gilbert); to whose early, active and liberal patronage, in conjunction with the efforts of Dr. Paris, it first owed its existence, and by its connexion with his name its labours have been more extensively known, and far more generally acknowledged, than they might otherwise have been.

The Council would fain have indulged in *more grateful* allusions, than a bare official form permits, to the numerous advantages and benefits the Society has owed to his kind and continued benefactions during the twenty-six years it was honoured by his presidency over its affairs—but that they have been anticipated by other pens—yet they cannot look back on his kind and considerate conduct without the feeling that every member of the Society has, by his loss, a friend less in the world.

At the foundation of the Society, twenty-seven years ago, the value of, and necessity for scientific education among our practical miners was barely thought of; yet it was among the very first and most important objects of its founders: and to them it is the sincerest matter of gratulation that a subject so often enforced from its chair, and recommended by its patrons and in its reports, is at length recognized as one of paramount utility and importance.

This has been shown, not only by the foundation of Professorships for civil and mining engineering in the academic institutions of London and Durham, but by the institution of a Mining School in Cornwall, which, although first thought of as the result of a due appreciation of the public virtues of one of the earliest and most munificent friends of this Society (the late Lord de Dunstanville), has been carried into practical effect by the liberality of Sir Charles Lemon, by whose enlightened and patriotic exertions its permanent existence will, we trust, be secured. We look forward to the period when the result of the studies pursued in this institution, united to the extensive practical knowledge to be derived from exploring the variety of our rocks,—the different characters of our "*lodes*," and the vast mechanical powers employed in working our mines,—will raise our miners far above their present position, although even now they bear the character (which they richly deserve) of the best and most useful practical miners in the world.

Much has been said of a reception-room for plans,—and such a depository has from the foundation of the Society been opened here, and to some extent made useful by the liberality and intelligence of some of our mine agents. But when the labour of preparing them—highly valued as they are—and the want of adequate remuneration, are considered, we need not wonder at the comparative

poverty of our archives in this respect. Mr. Henwood's Survey of the Mines has probably brought together a larger collection of copies of mining plans than had ever before been obtained; but unless similar labour be still devoted to their accumulation, it is vain to hope for them, unless at an expense which would perhaps but seldom be repaid by their value to any but practical miners.

The rock formations of Cornwall had, until recently, been thought among the most ancient; but the late researches of Messrs. Sedgwick and Murchison induce them to place our "*killas*" at an epoch not anterior to the old red sandstone, on zoological evidence of much force; whilst to the granite and its congeners they ascribe a still more recent date. From the labours of these eminent philosophers, associated with the visit of Prof. Phillips (under the direction of the Government), and, in some humble degree, aided by the efforts of our own members, we hope this obscure portion of geological investigation will receive an elucidation equally luminous with that which Mr. Murchison's herculean labours have shed on the closely-allied rocks of the "*Silurian*" region.

The rapidly-accumulating collections of the Society are even now more than sufficient to fill every species of accommodation the present museum affords; and it will be imperative on your new officers and Council to devise a method for rendering them more generally available and useful than their present circumscribed premises will permit.

It has been suggested, that with so much geological wealth as the Society possesses, the benefits it confers are but limited; and with a view to repeating an experiment which was unsuccessfully made by the late zealous and excellent Secretary (Dr. Boase), a few lectures will from time to time be given by one of the officers during the ensuing winter; their periods will, however, be determined by engagements in which the Society has no part.

It had been confidently anticipated that the Fifth Volume of the Society's Transactions would, ere now, have been in the hands of the members: considerable progress has been made in it during the present year, but its completion has been delayed by professional engagements of the editor; the Council, however, believe that it will be published at an early period of the ensuing season.

By order,

W. J. HENWOOD,

Secretary and Curator.

October 9th, 1840.

The following papers have been read since the last Report:—

I. On some singular Metalliferous Deposits in the Mining district near St. Ives, called by the miners "*Carbona*." By Joseph Carne, Esq., F.R.S., F.G.S., M.R.I.A., &c., &c., Treasurer of the Society.

II. Remarks on the Land-slip between Axmouth and Lyme Regis. By the Rev. Canon Rogers, A.M., Member of the Society.

III. Notes on the Geology of the Counties of Gloucester and Restigouche, in New Brunswick, and the Canadian bank of the river Restigouche. By W. J. Henwood, C.E., F.R.S., F.G.S.S. London

and Paris, Hon. M.Y.P.S., Secretary of the Society, and Curator of the Museum.

IV. Observations on a suite of Specimens from the neighbourhood of Exeter. By Joseph Parker, Jun., Esq., Corresponding Member of the Society.

V. On the Age of the Shingle Beach at Pevensey, in Sussex. By John S. Enys, Esq., F.G.S., &c., Member of the Society.

VI. On the Organic Remains contained in the Slates and Limestones of South Devon. By J. C. Bellamy, Esq., Curator of the Devon and Cornwall Natural History Society.

VII. On the Occurrence of Organic Remains in the Slate Rocks of the Southern Coasts of Cornwall. By Charles W. Peach, Esq., Associate of the Society.

VIII. On the Sulphur Ores (iron pyrites) of the Vale of Ovoca, county of Wicklow. By W. J. Henwood, C.E., F.R.S., Corresponding Member of the Plymouth Institute.

The Curator's Report notices the following Donations to the Museum :—

Cinnabar, with hematite iron ore and copper pyrites from Bavaria, vitreous and purple copper ores and copper pyrites from Kenmare mine, county of Kerry, and metallic copper precipitated on the pumps of Connorree mine, county of Wicklow. By Thomas Cornish, Esq.—Specimens from the recent land-slip on the coast between Axmouth and Lyme. By the Rev. Canon Rogers, A.M., Member of the Society.—Specimens from the trap dyke, and of the accompanying rocks, from Mr. Pennant's slate quarry at Penrhyn, Caernarvonshire. By Joseph Carne, Esq., F.R.S., F.G.S., M.R.I.A., &c., Treasurer of the Society.—Wood-tin and garnets, from Polberrow mine, St. Agnes. By John T. Tregellas, Esq.—Pseudomorphous quartz, from Caradon, and crysocola, from near Five Lanes. By Mr. George Jennings, Jun.—Stream-tin ore from Carnon mine. By Mr. Nicholas S. Cloak.—Pearl spar and iron pyrites from Trevaskus mine. By Mr. — Joseph.—Galena, copper pyrites and sulphuret of antimony from Sicily. By — Floyd, Esq.—A suite of specimens from the neighbourhood of Exeter, and organic remains resembling Alcyonia, from the new red sandstone of Devon. By Joseph Parker, Jun., Esq., Corresponding Member of the Society.—Organic remains from the slate series and limestones near Plymouth. By J. C. Bellamy, Esq., Curator of the Devon and Cornwall Natural History Society.—A fine specimen of Jew's-house tin from St. Austell. By John Michell, Esq.—Chert from Halkin mountain, Flintshire. By Richard Thomas, Esq. (of London).—Organic remains from the summit of Snowdon, and carbonate of manganese from Caernarvonshire. By Henry Thomas, Esq., F.G.S., Associate of the Society.—A suite of specimens from the coast of Antrim and other parts of Ireland. By C. A. Johns, Esq.—Native copper, crystallized copper pyrites, and iron pyrites from Providence mines, near St. Ives; galena from North Wheal Alfred, with specimens from the copper and sulphur (iron pyrites) ores of Wicklow. By Mr. Higgs, Member of the Society.—Chalcedony, organic re-

mains from the chalk, and conglomerates from near King's Langley, Herts. By H. Campbell White, Esq., F.G.S., &c., &c., &c.—Quartzose slickenside and purple copper ore from Carn Brea mines, crystals of the red oxide of copper, silicate of tin from Weald Coats, copper pyrites and carbonate of iron from Wheal Tolgus, and a new ore of copper from Great Saint George mine. By John Garby, Esq., Associate of the Society.—Crystalline quartz from Knockmahon mine, county of Waterford. By John Petherick, Esq.—Acicular oxide of copper from Knockmahon mine. By Captain James Clemes.—Conglomerate from Slievnamann mountain, county of Tipperary, and sandstone from Mohir, county of Clare. By Day P. Le Grice, Esq., Member of the Society.—Specimens from various localities. By the Rev. Henry Holden.—Organic remains from Fowey and Caerhayse. By C. W. Peach, Esq., Associate of the Society.—Vitreous and purple copper ores, and copper pyrites from Kenmare mine, county of Kerry. By Dillon Croker, Esq.—Recent sandstone from Lelant. By the Rev. W. D. Longlands.—Wood from the diluvium at St. Erth stream. By Mr. Samuel Peters.—Hematite iron ore from Launceston, Van Diemen's Land. By Richard Edmonds, Esq.—Specimens from the neighbourhood of Killarney, county of Kerry, from Knockmahon mine, county of Waterford, and from Cronebane, Tigrony, Connorree, Ballymurtagh, and Ballygahn, in the Vale of Ovoca, county of Wicklow. By W. J. Henwood, C.E., F.R.S., F.G.S., Secretary of the Society and Curator of the Museum.

Officers and Council for the present year :—

President.—Sir Charles Lemon, Bart., M.P., F.R.S., &c. *Vice-Presidents.*—Sir T. D. Acland, Bart., M.P., F.R.S.; John Paynter; John Taylor, F.R.S., &c.; Stephen Davey. *Treasurer.*—Joseph Carne, F.R.S. *Joint Secretaries.*—Samuel Pidwell, Jun.; W. J. Henwood, F.R.S. *Librarian.*—Richard Hocking. *Council.*—John Batten, John J. A. Boase, Thomas S. Bolitho, Samuel Borlase, Charles Fox, Thomas Lean, J. N. R. Millett, Rev. M. N. Peters, W. Petherick, N. Phillips, William Reynolds, W. M. Tweedy.

LXXI. *Intelligence and Miscellaneous Articles.*

ATOMIC WEIGHT OF CARBON.

MM. DUMAS and Stas have, together, performed fourteen experiments relative to the atomic weight of carbon; the results all agree, and were obtained either by the combustion of pure charcoal or of highly carbonated and well-known substances. The combustion was performed in oxygen, and care was taken to dry the gases obtained either by sulphuric acid or chloride of calcium. Thus dried, they were passed through two pieces of apparatus filled with solution of potash, and a third filled with potash in powder. The increased weight of the solutions and dry potash gave the weight of the carbonic acid obtained. Thus the weight of the carbon burnt, and of the carbonic acid gas obtained, were known; and from these, with-

out any hypothesis, the proportions in which the bodies combine could be deduced. According to M. Berzelius, the proportions are 200 of oxygen to 76.52 of carbon. According to the recent experiments, above-mentioned, by MM. Dumas and Stas, the result will be very different, for they gave by the combustion of naphthalin, four experiments, 75.21, 75.01, 75.08, 75.07; by the combustion of camphor, three experiments, 75.1, 75.1, 75.0; by the combustion of benzoic acid, two experiments, 75.09, 75.03; by that of the native graphite of Ceylon, three experiments, 74.91, 75.05, 74.99; by artificial graphite extracted from an iron which contained most of it, two experiments, 74.87, 74.90. "All these numbers," M. Dumas remarks, "agree in showing that the true atomic weight of carbon is 75, and not 76.52. There is consequently an error in the indispensable elements in fixing the formulæ now employed in organic chemistry. That is to say, there will be many formulæ to modify, many analyses to repeat, especially of those substances which are rich in carbon, in which very considerable errors may have been committed."

M. Dumas adds, that the Academy will remark with interest, that this long and laborious series of experiments has brought us to the atomic weight indicated by Dr. Prout, who had long supposed that the atomic weight of charcoal was exactly equal to six times that of hydrogen, or $12.5 \times 6 = 75$, which is the number given by the mean of our results. If, as believed by Prout, and as now appears very probable, all atomic weights are multiples of that of hydrogen by whole numbers, there will be many things to rectify in the atomic weights at present adopted. Future experiment will decide this point, but it is evident that they must be submitted to a serious verification.

"The Academy," continues M. Dumas, "will remark also with interest, that the atomic weight of carbon which results from these experiments agrees much better than the former with the old analyses of Iceland spar, arragonite and marble, made by Thenard and Biot, as well as with the densities of oxygen and carbonic acid, determined either by MM. Biot and Arago, or by M. de Saussure, whose results also approximate to ours with regard to the combustion of charcoal."

M. Boussingault has communicated some analyses of bitumen, which entirely agree with our results."—*L'Institut*, No. 347.

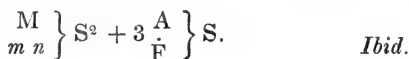
PYRRHITE—A NEW MINERAL.

Only one example of this substance is known, and occurs in a splendid drusy cavity of felspar, which is in the possession of Vice-President Perowski, of Petersburg. While the cavity chiefly contains felspar crystals several inches in size, finely defined, and of an ochre-yellow colour, it likewise includes six-sided tables of reddish-white, pearly lithion mica; white translucent crystals of albite; crystals of clove-brown rock-crystal; and a few white topazes. The crystals of the new mineral are superimposed on the felspar, are

eight in number, and are octahedrons of about three lines in length. Their surfaces are smooth, but possess little lustre, so that their angles cannot be measured with great accuracy; but from observations made on several angles, the mean may be regarded as $109^{\circ}28'$, so that we may probably assume that the crystals are regular octahedrons. No cleavage is observable. The colour is orange-yellow, and the lustre feebly vitreous. The substance is translucent on the edges, its hardness is that of felspar, but the specific gravity could not be determined. It occurs at Alabaschka, near Mursink, and on account of its yellow colour has been named Pyrrhite.—*Jameson's Journal*, July 1840.

PIHLITE—A NEW MINERAL.

Sefström has discovered at Fahlun a new mineral which replaces mica in granite, and which he has termed Pihlite, in honour of the late M. Pihl, Director of Mines. It is an intermediate substance between talc and mica, and its composition is expressed by the following formula:



DYSODIL.

This mineral, arranged in systems of mineralogy under the name of *Foliated Mineral Pitch*, Ehrenberg has shown to consist of bitumen, or mineral pitch, mixed with siliceous shells of infusoria, and occasionally with pollen of pines, &c. The wax-yellow variety found in Sicily, is made up of shells of *Naviculæ* and mineral pitch: the nearly black-brown coal of the Westerwalde, is a variety of dysodil; so also is the foliated leather-like bituminous coal of the Geistingner Busch at Rott and Siegburg in the Siebengebirge, and a foliated brown coal of the Vogelsberge. Hence the mineral species named dysodil appears to be a polir-slate impregnated with bitumen. Its colours are black-brown, or black. It never forms very thick beds, but sometimes widely-spread deposits. It is used as fuel.—*Annals of Nat. History*, April 1840.

METEOROLOGICAL OBSERVATIONS FOR OCT. 1840.

Chiswick.—October 1. Overcast. 2, 3. Very fine. 4. Rain. 5. Fine: rain. 6. Fine. 7. Frosty and foggy. 8. Very fine. 9. Hazy. 10. Dense fog: very fine. 11. Hazy. 12—15. Foggy in the mornings: fine. 16. Overcast. 18. Cloudy: rain. 19. Cloudy. 20. Clear. 21. Fine. 22. Hazy: rain. 23. Overcast: rain. 24. Overcast. 25. Very fine. 26. Overcast. 27. Heavy rain: clear. 28. Fine. 29. Foggy: rain: dense fog at night. 30. Cloudy and fine: clear. 31. Foggy: clear at night.

Boston.—Oct. 1, 2. Cloudy. 3. Fine. 4. Cloudy. 5. Cloudy: rain early A.M. 6, 7. Fine. 8. Fine: rime frost this morning. 9. Cloudy. 10, 11. Fine. 12—14. Foggy. 15. Fine. 16. Cloudy. 17. Rain: rain early A.M. 18. Cloudy: rain P.M. 19. Stormy. 20. Fine. 21. Cloudy. 22. Cloudy: rain early A.M. 23. Fine: rain P.M. 24. Fine. 25. Fine: rain early A.M. 26. Fine. 27. Cloudy: rain early A.M. 28. Foggy. 29. Cloudy: rain A.M. and P.M. 30, 31. Foggy.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTSON; by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. VEALL at Boston, and by Mr. DUNBAR at Applegarth Manse, Dumfries-shire.

Days of Month, 1840.	Barometer.			Thermometer.				Wind.			Rain.			Dew point.	
	Chiswick.		Dumfries-shire.	London : Roy. Soc.		Dumfries-shire.		London: Roy. Soc.		Dumfries-shire.	Chiswick.	London: Roy. Soc.	Dumfries-shire.	Dumfries-shire.	Dumfries-shire.
	Max.	Min.		Fahr. 9 a.m.	Self-register. Max. Min.	Max.	Min.	Max.	Min.						
			9 a.m.	8½ p.m.											
			The Meteorological Observations from Applegarth Manse, Dumfries-shire, for October, have not yet been received.												
1.	30.022	30.039	29.996	29.34	55.3	57.7	51.7	58	46	53.5					50
2.	30.172	30.124	30.039	29.56	53.7	58.7	51.3	61	36	51					52
3.	30.194	30.153	30.061	29.66	47.7	59.0	44.3	56	46	44					46
4.	30.084	30.056	30.033	29.60	51.2	54.6	46.3	57	45	53					49
5.	30.100	30.073	29.997	29.54	49.5	57.2	46.4	57	33	50					48
6.	30.126	30.094	30.082	29.60	45.4	55.3	42.6	53	30	44					45
7.	30.114	30.121	30.071	29.53	43.7	53.6	39.0	58	30	47					42
8.	30.266	30.241	30.190	29.70	40.7	53.9	36.8	60	30	43					41
9.	30.326	30.293	30.260	29.75	40.3	51.6	40.0	59	30	48					39
10.	30.318	30.280	30.254	29.79	45.7	55.4	39.8	60	35	45.5					41
11.	30.400	30.401	30.323	29.92	52.5	56.7	44.4	58	35	40					46
12.	30.604	30.549	30.543	30.11	50.3	59.3	44.7	60	31	39					46
13.	30.590	30.548	30.328	30.05	43.2	57.3	42.2	63	31	40					43
14.	30.404	30.359	30.271	29.83	43.2	56.6	39.7	66	32	45					42
15.	30.258	30.225	30.071	29.69	43.7	60.7	41.9	57	36	41.5					42
16.	29.936	29.915	29.755	29.34	48.3	54.8	42.0	56	51	49					46
17.	29.812	29.757	29.736	29.36	54.8	56.7	48.0	56	46	48.5					49
18.	30.026	29.977	29.755	29.20	50.8	56.8	49.0	56	49	49					47
19.	29.704	29.803	29.664	29.04	53.5	55.7	49.0	57	42	50.5					48
20.	30.100	30.075	30.052	29.58	47.7	56.6	45.8	57	29	46					45
21.	30.156	30.124	30.046	29.65	45.7	54.6	42.2	54	39	42					41
22.	29.904	29.897	29.861	29.40	48.8	52.8	44.8	53	38	45					45
23.	29.960	29.933	29.662	29.43	45.2	52.7	43.7	53	40	41.5					42
24.	29.556	29.586	29.523	29.06	45.2	53.2	42.5	53	35	42					43
25.	29.784	29.877	29.746	29.33	42.2	50.6	39.3	51	28	40					38
26.	29.886	29.875	29.126	29.40	39.2	49.3	35.4	52	43	38					37
27.	29.178	29.204	29.160	28.75	51.3	52.8	38.6	50	30	49					45
28.	29.136	29.266	29.114	28.76	42.7	52.3	41.3	52	36	37.5					42
29.	29.340	29.369	29.292	29.00	45.6	49.4	41.4	50	32	45					43
30.	29.522	29.520	29.477	29.15	47.3	49.6	40.3	56	36	42					44
31.	29.564	29.566	29.515	29.19	47.3	52.8	43.2	57	36	47					45
Mean.	29.985	29.984	29.871	29.46	47.2	54.8	43.2	56.32	36.64						Mean.
												Sum.	1.35	1.63	

THE
LONDON, EDINBURGH AND DUBLIN
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SUPPLEMENT TO VOL. XVII. THIRD SERIES.

LXXII. *Remarks on Professor Challis's Investigation of the Motion of a Small Sphere vibrating in a Resisting Medium.*
By GEORGE BIDDELL AIRY, Esq. M.A., F.R.S., Astronomer Royal.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN your Number for December there is a paper by my friend Professor Challis, on the theoretical resistance to the motion of a sphere vibrating in an elastic medium. The problem is so difficult, and so important in its application to geodesy, and therefore of such general interest, that I have thought it best to state, in a public communication, the difficulty which I feel with regard to one step of the investigation, and to request Professor Challis to remove my difficulty by communication to your journal.

I see nothing liable to objection in pages 463 and 464; but with the top of page 465 my difficulty commences. The differential equation, tacitly used by Professor Challis, is that

whose solution is $v = \frac{f'(r-at)}{r} - \frac{f(r-at)}{r^2}$; which equation

is perfectly correct for waves, diverging with equal intensity and with corresponding phase, in all directions from a centre; or, if not in all directions, it is yet true if the waves diverge with equal intensity and corresponding phase through all the angular directions included in a spherical sector bounded by material planes, which (produced if necessary) would meet at the centre of the sphere. But it is not true in any other case. Thus we may have two such spherical sectors, separated only by a material partition, and with waves of different intensities and non-corresponding phases propagated in the two sectors, from the centre, or from the surface of a small concentric sphere; and the equation applies to each sector separately; but if the partition be removed it no longer applies to the whole compounded sector. For the pressures against the partition, produced by the fluids on the two sides, were different; and, therefore, on removing the partition, a new motion of a

Phil. Mag. S. 3. Vol. 17. No. 113. *Suppl. Jan.* 1841. 2 I

totally different kind, modifying the old pressures, and therefore modifying the old motions, will be introduced. This remark applies even when the phases correspond, if the intensities are different.

Now it appears to me, as far as I can follow the investigation, that some such process has been used as adopting the solution above alluded to, and supposing it to hold with waves of different intensities in different parts of the surface of the small sphere. For the expressions in lines 25 and 28 contain the factor $\cos \theta$, which, as it is not differentiated in the operation for forming $\frac{du}{dt}$, but remains still as a factor, seems to imply that the wave in each infinitesimal sector goes on just as if there were no other sector near it communicating lateral pressures.

If I am correct in the view which I have taken of the connexion of these steps of the process, I conceive that the investigation must be considered faulty. Of the truth of my first remarks I have no doubt; but I am less confident as to the exact connexion of the different steps in Professor Challis's investigation; and upon this point I am anxious to be informed.

I am, Gentlemen,

Your very obedient servant,

Royal Observatory, Greenwich, December 9, 1840.

G. B. AIRY.

LXXIII. *Address of the General Secretaries of the British Association*, RODERICK IMPEY MURCHISON, F.R.S., F.G.S., and Major EDWARD SABINE. V.P.R.S.: read at the Meeting at Glasgow, September 1840.

[Continued from p. 449, and concluded.]

FROM Zoological researches let us now turn to Physical Geology. One of the most interesting fruits of modern experimental research is the knowledge of the fact, that electrical currents are in continual circulation below the surface of the earth. Whether these currents, so powerful in developing magnetical and chemical phenomena, are confined to mineral veins and particular arrangements of metal and rock, or generally capable of detection by refined apparatus well applied, appeared a question of sufficient importance to deserve at least a trial on the part of the Association. Our present volume records the result of such a trial on the ancient and very regularly stratified rocks of Cumberland, consisting of limestone, sandstone, shale, and coal, so superimposed in many repetitions as to resemble not a little the common arrangement of a voltaic pile. Varied experiments, with a galvanometer of con-

siderable delicacy, failed to detect, in these seemingly favourable circumstances, any electrical current.

The extensive and rapidly increasing applications of iron to public and private structures of all kinds in which durability of material is a first requisite, have made it highly desirable to possess accurate information respecting the nature of the chemical forces which effect the destruction of this hard and apparently intractable metal. The preservation of iron from oxidation and corrosion is indeed an object of paramount importance in civil engineering. The Association was, therefore, anxious to direct inquiry to this subject, and gladly availed itself of the assistance of Mr. Mallet, a gentleman peculiarly qualified for such investigations, both from his knowledge as a chemist, and from his opportunities of observation as a practical engineer. An extensive series of experiments has accordingly been instituted by him, with the support of the Association, on the action of sea and river water, in different circumstances as to purity and temperature, upon a large number of specimens of both cast and wrought iron of different kinds. These experiments are still in progress, and the effects are observed from time to time. They will afford valuable data for the engineer, and form the principal object of the inquiry; but a period of a few years will be required for its completion. In the meantime, Mr. Mallet has furnished a report on the present state of our knowledge of the subject, drawn from various published sources, and from his own extensive observations. In this report he examines very fully the general conditions of the oxidation of iron, and how this operation is greatly promoted, although modified in its results, by sea-water; also in what manner the tendency to corrosion is affected by the composition, the grain, porosity, and other mechanical properties of the different commercial varieties of iron. The influence of minute quantities of other metals, in imparting durability to iron, is also considered. Mr. Mallet devotes much attention to the consequences of the galvanic association of different metals with iron, a subject of recent interest from the applications of zinc and other metals to protect iron, which are at present agitated. He concludes this, his first report, by recommending a series of inquiries, ten in number, which will supply the desiderata immediately required by the engineer and by the chemist.

We have next to notice a report by Professor Powell, on the present state of our knowledge of refractive Indices for the standard rays of the solar spectrum in different media. The difficulty which the fact of the dispersion of light has offered to the universal application of the undulatory theory,

has been in a great measure removed by the analysis of Cauchy and others, who have considered the distances of the undulatory particles as quantities comparable to the length of a wave; velocities of propagation of the different rays of the spectrum are made to depend upon the length of wave which constitutes a ray of a given colour, and upon certain constants proper to the medium; these constants being obtained from observations on refractive indices for certain definite rays (or dark lines) of the spectrum, the refrangibility of any other definite ray (whose wave-length has been ascertained by examining an interference-spectrum) becomes known, and may be compared with observation as a test of theory; such experiments have been made by Fraunhofer, Rudberg, and Professor Powell, who has given a tabular view of the various results, without, however, instituting the comparison between theory and observation, which it would be desirable to extend further than has yet been done. It would be important also to elucidate the disturbing effect of temperature, which prevents even existing observations from being rigorously comparable.

The calculations respecting the tides, which have been prosecuted by the aid of the Association ever since its institution, have been continued this year by Mr. Bunt, under the directions of Mr. Whewell. These calculations have now reached such a point, that the mathematician, instead of being, as at the beginning of this period, content with the first rude approximations, is now struggling to obtain the last degree of accuracy.

The country in which we are now assembled, has always been conspicuous for attention to meteorology, a branch of physical science, in which the British Association, with its power of combining the efforts of many observers in distant quarters of the globe, may hope to be especially useful.

In Scotland, Leslie opened a new train of inquiry, by examining the earth's temperature at different depths; and his successor in the University of Edinburgh, is now directing, at the request of the Association, a large and complete course of experiments on that interesting subject. Framed in conformity with the plans adopted for similar objects by Arago and Quetelet, these researches of Professor Forbes contain also the means of determining the power of conducting heat, which different sorts of rock possess; and may thus throw light on some of those peculiarities in the distribution of temperature at greater depths below the surface, which have become known by experience, but are not explained by theory.

In Scotland, Sir David Brewster was the first to obtain an hourly meteorological journal for a series of years, and to draw from that fertile source new and important deductions, which

have had a powerful influence on the progress of scientific meteorology. How gratifying to receive, through the same hands, after the lapse of nearly 15 years, an additional contribution of the same kind, and from the same country; but embracing new conditions, on a new line of operations, in order to obtain new results! By the observations now in progress at Inverness, and at Kingussie, the influence of elevation in modifying the laws which have been found to govern the hourly distribution of heat near the level of the sea, may be discovered, and thus a great addition be made to the experimental results, for which science has long been grateful to the distinguished philosopher we have named, and which have been described as "of the highest value to meteorology, and as the only channel through which any specific practical information can be obtained in this most interesting department of physics."

This is no ordinary praise. It is the just tribute of one who is worthy to offer it; one, who at the call of the British Association, has conducted at Plymouth a still more extensive series of similar observations, and has added to them hourly comparisons of the temperature and moisture of the air, and an hourly record of barometric oscillations. Mr. Snow Harris has presented in a few pages of our last report, the precious results of (70,000) observations, and thus rendered them immediately available in the foundations of accurate meteorology. The documents thus patiently collected, are, however, not yet exhausted in value; they may be again and again called into the court of science, and made to yield testimony to other, and as yet, unsuspected truths. They must not be lost. Shall we lay them by in manuscript among other unconsulted records of the past labours of men, or by undertaking their publication, do justice to our workmen, and establish a new claim on the imitation of the present, and the gratitude of future days? This question is of serious import. Already, stimulated by success in thermometric registration, we have set to work on a more perplexing problem; we have resolved to bind even the wandering winds in the magic of numbers. While we speak, the beautiful engines of our Whewells and Oslers are tracing at every instant of time, the displacements of the atmosphere at Cambridge, at Plymouth, at Birmingham, in Edinburgh, in Canada, in St. Helena, and at the Cape of Good Hope; and ere long we may hope to view associated in one diagram, the simultaneous movements of the air over Europe, America, Africa, India, and Australia, recorded with instruments which we have chosen, by men whom we have set to work.

Among the causes which tend to retard the progress of

science, few, perhaps, operate more widely than the impediment to a free and rapid communication of thought and of experiments, occasioned by difference of language. It appeared to the British Association, that this impediment might in some degree be removed, as far as regards our own country, by procuring, and causing to be published, translations of foreign scientific memoirs judiciously selected. Accordingly at each of the meetings at Newcastle and Birmingham a grant of 100*l.* was placed at the disposal of a committee appointed to carry this purpose into effect. Aided by the contributions of several translations which have been gratuitously presented to them, the committee have been enabled, in the two last years, to publish fourteen memoirs on subjects of prominent interest and importance in the mathematical and physical sciences, bearing the names of some of the most eminent of the continental philosophers.

Such, gentlemen, is an imperfect review of our recent proceedings. In two essential respects the British Association differs from all the annual scientific meetings of the Continent, no one of which has printed transactions or employed money in aiding special researches. We also differ from them in the communications which, in the name of the representatives of science assembled from all parts of the United Kingdom, we feel ourselves authorized to make from time to time to the Government, on subjects connected with the scientific character of the nation. On our first visit to Scotland, for example, we felt it to be an opprobrium that this enlightened kingdom should, in one essential feature of civilisation, be still behind many of the continental states, and we prepared an address to his late Majesty's Government, urging strongly the necessity of the construction, without delay, of a map of Scotland, founded on the trigonometrical survey. Representations to the same effect have since been made by the Royal Society of Scotland, and by the Highland Society, and the subject has now engaged that attention, which will, we trust, soon procure for this country the first sheets of a large and complete map.

If then it be asked, why are the men of highest station happy to associate and mingle with us in official duties?—why have the heads of the noble houses of Fitzwilliam, Lansdowne,* Northampton, Burlington, Northumberland, and Breadalbane, alternated in presiding over us, with our Bucklands, our Sedgwicks, our Brisbanes, our Lloyds, and our

* The Marquis of Lansdowne, who had accepted the office, was prevented from attending by deep domestic affliction, and the Marquis of Northampton cheerfully supplied his place.

Harcourts?—why indeed, on this very occasion has Argyll himself, overlooking the claims due to his high position, and his ancient lineage, come forward to act with us, and even to serve in a subordinate office?—may we not reply, that it is, we believe, a consequence of the just appreciation on the part of these patriotic and enlightened noblemen, of the beneficial influences which this Association exercises in so many ways on the sources of the nation's power and honour?

If we have hitherto dwelt almost exclusively on the value of our transactions, researches, recommendations, and the good application of our finances, let it not, however, be supposed, that we are not also fully alive to the advantages which flow from the social intercourse of these meetings, by bringing together, into friendly communion, from distant parts, those who are struggling on (often remote and unassisted) in advancing experimental science. If, indeed, this principle of union (which we are proud to have borrowed from our German brethren) has been hitherto found to work so well amongst our own countrymen, we cannot but doubly recognise its value when we see assembled so many distinguished persons from foreign countries. In the presence of these eminent men, we forbear to allude to individual distinctions, conscious that any brief attempt of our own would fall far short of a true estimate of merits, the high order of which is indeed known to every cultivator of science in Britain. Well, however, may we rejoice in having drawn such spirits to our Isle; valuable, we trust, will be the comparisons we shall be enabled to make between the steps which the different sciences are making in their countries and in our own.

That advantages, indeed, of no mean order arise from such social intercourse, is a feeling now so prevalent, that foreign national associations for the promotion of natural knowledge have rapidly increased. Germany, France, and Italy have their annual Assemblies, and our allies of the Northern States hold their sittings beyond the Baltic. In all this there is doubtless much good, but an occasional more extensive intercourse of a similar nature, to be repeated at certain intervals, is greatly to be desired.

It has therefore appeared to us (and we say it after consultation with many of our continental friends, who equally feel the disadvantage), that the formation of a general congress of science might be promoted at this meeting, which, not interfering with any assemblies yet fixed upon, or even contemplated, may be so arranged, as to permit the attendance of the officers and active members of each national scientific institution.

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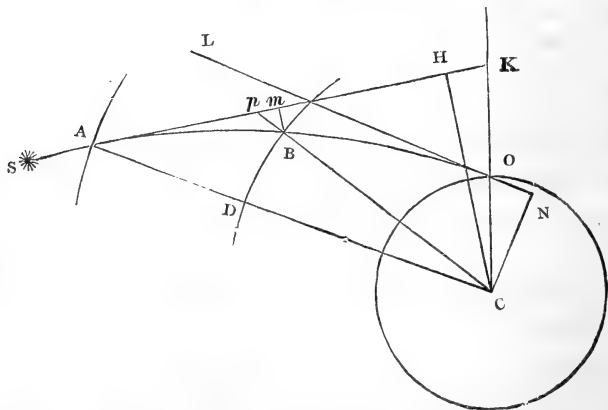
If the British Association should take the first step in proposing a measure of this kind, and should solicit the illustrious Humboldt to act as President, we are sure that scientific men of all nations would gladly unite in offering this homage to a man whose life and fortune have been spent in their cause, whose voice has been so instrumental in awakening Europe to the inquiry into the laws of terrestrial magnetism, and whose ardent search after nature's truths has triumphed over the Andes and the Altai.

If such be your suggestion, then will a fresh laurel be added to the wreath of this city. She who, through the power bequeathed to her by her illustrious offspring, conveys with rapid transit her inventions and her produce to the remotest lands, well can she estimate the value of an union of men whose labours can but tend to cement the bonds of general peace. In such a body the British representatives would, we trust, form no inconspicuous band; and with minds strengthened by the infusion of fresh knowledge, they would, on re-assembling for our own national ends, the better sustain the permanent and successful career of the *British Association*.

LXXIV. *On the Heat of Vapours and on Astronomical Refractions.* By JOHN WILLIAM LUBBOCK, Esq., *Treas. R.S., F.R.A.S. and F.L.S., Vice-Chancellor of the University of London, &c.*

[Continued from p. 473, and concluded.]

IF the constitution of the atmosphere be such as I have concluded, by proper substitutions in the differential equations of refraction, an accurate table of refractions is to be



procured, which may be compared with that of M. Bessel obtained empirically.

Let S A O N be the trajectory described by light emanating from the star S in its passage through the atmosphere to the earth's surface at O, θ the apparent zenith distance, or the angle which the tangent to the trajectory makes with the line C O K at O, C H perpendicular to S A K, the direction of the ray before it enters the atmosphere = y , $a = C O$, then

$$d. \delta \theta = \frac{d y}{\sqrt{(a + z)^2 - y^2}}$$

$$y = a \sin \theta \sqrt{\frac{1 + 2 K \rho}{1 + 2 K \rho'}}$$

$$* \alpha = \frac{K \rho}{1 + 2 K \rho} \quad y = \frac{a \sin \theta}{\sqrt{1 - 2 \alpha \omega}}$$

I assume these equations, which are proved by Mr. Ivory in the *Phil. Trans.*, 1838†, and which are equivalent to similar equations given in the *Méc. Céleste*.

$$d. \delta \theta = \frac{\alpha \sin \theta d \omega}{(1 - 2 \alpha \omega) \sqrt{\cos^2 \theta + \left(\frac{z^2}{a} + \frac{z^3}{a^2}\right) (1 - 2 \alpha \omega) - 2 \alpha \omega}}$$

$$\frac{z}{1 + \frac{z}{a}} = a i u. \quad i \text{ being a constant and } u \text{ a certain function of the}$$

density, which depends upon the constitution of the atmosphere, and which for the present may remain undefined.

$$\frac{z^3}{a} + \frac{z^2}{a^2} = 2 i u + 3 i^2 u^2 + \&c.$$

$$d. \delta \theta = \frac{\alpha \sin \theta d \omega \{1 + 2 \alpha \omega + \&c.\}}{\sqrt{\cos^2 \theta + 2 i u + 3 i^2 u^2 + \&c. - 2 \alpha \omega}}$$

$$\text{if } x = u - \frac{\alpha}{i} \omega, \quad i u - \alpha \omega = i x \quad \omega = 1 - \frac{g}{g'}$$

$$2 i u + 3 i^2 u^2 + \&c. - 2 \alpha \omega = 2 x + 3 x^2.$$

“The quantities rejected being plainly of no account relatively to those retained. Further, because ω is always less than 1, $\frac{\alpha}{1 - 2 \alpha \omega}$ is contained between α and $\alpha (1 + 2 \alpha)$, and it may be taken equal to α , or to the mean value $\alpha (1 + \alpha)$ ‡.” Thus we have (See *Phil. Trans.* 1838. p. 205.)

* This quantity must not be confounded with the α which accompanies θ .

[† Mr. Ivory's paper here referred to has been reprinted in *L. & E. Phil. Mag.*, beginning in vol. xv. p. 3, and concluding in vol. xvi.—*EDIT.*]

‡ Laplace introduces the same simplification. *Méc. Céleste*, vol. iv. p. 24.

$$\begin{aligned}
 d.\delta\theta &= \sin\theta \times \frac{\alpha(1+\alpha)d\omega}{\sqrt{\cos^2\theta + 2ix + 3i^2x^2}} \\
 &= \sin\theta \times \frac{\alpha(1+\alpha)d\omega}{\sqrt{\cos^2\theta + 2ix}} \\
 &\quad [1] \\
 &\quad - \frac{3}{2} \frac{\sin\theta \alpha(1+\alpha)i^2x^2d\omega}{(\cos^2\theta + 2ix)^{\frac{3}{2}}} + \&c. \\
 &\quad [2]
 \end{aligned}$$

The refraction will thus consist of two terms, which I proceed to consider separately. The second term is minute, not amounting to 2'' sex. at the horizon.

Throughout this treatise on Astronomical Refractions one accent will be affixed to any symbol that it may denote the particular value of the variable which obtains at the surface of the earth, and two accents will be affixed when the particular value which obtains at the superior limit of the atmosphere is intended.

The limits of x , or x' and x'' corresponding to u' and u'' , are $x' = 0$, $x'' = u'' - \frac{\alpha}{i}$, because $\omega' = 0$, $\omega'' = 1$.

$x = u - \frac{\alpha}{i}\omega \Leftarrow u - \frac{\alpha}{i}f u$; the function indicated by the letter f for the present may remain undefined.

By Lagrange's theorem

$$\begin{aligned}
 u &= x + \frac{\alpha}{i}f x + \frac{\alpha^2}{2i^2} \frac{d(fx)^2}{dx} + \frac{\alpha^3}{2.3i^3} \frac{d^2(fx)^3}{dx^2} + \&c. \\
 &= x + \frac{\alpha}{i}\omega.
 \end{aligned}$$

Hence

$$\omega = f x + \frac{\alpha}{2i} \frac{d(fx)^2}{dx} + \frac{\alpha^2}{2.3i^2} \frac{d^2(fx)^3}{dx^2} + \&c.$$

Let $x = x'' - X$, $u = u'' - U$, $\omega = 1 - F U$, then

$$x'' - X = u'' - U - \frac{\alpha}{i} \{1 - F U\}$$

$$X'' = 0, \quad X' = x''$$

$$X = U - \frac{\alpha}{i} F U$$

$$U = X + \frac{\alpha}{i} F X + \frac{\alpha^2}{2i^2} \frac{d(FX)^2}{dX} + \&c.$$

$$\omega = 1 - F X - \frac{\alpha}{2i} \frac{d(FX)^2}{dX} - \frac{\alpha^2}{2.3i^2} \frac{d^2(FX)^3}{dX^2} - \&c.$$

This series may be used if the atmosphere extends only to a finite altitude.

Let

$$\sqrt{\frac{\cos^2 \theta + 2 i x}{i}} = z \quad \cos^2 \theta + 2 i x = i z^2$$

$$d x = z d z \quad x^2 = \frac{i^2 z^4 - 2 i z^2 \cos^2 \theta + \cos^4 \theta}{4 i^2}.$$

The integral of $d \cdot \delta \theta$ is to be taken from

$$z = \frac{\cos \theta}{\sqrt{i}} = z', \quad \text{to } z = \frac{\sqrt{\cos^2 \theta + 2 i x''}}{\sqrt{i}} = z''.$$

Let $\left(\frac{d^n \omega}{d x^n}\right)'$ represent the value of $\left(\frac{d^n \omega}{d x^n}\right)$ at the former of these limits, and $\left(\frac{d^n \omega}{d x^n}\right)''$ its value at the latter, then integrating continually by parts,

$$\int \frac{\alpha \sin \theta d \omega}{\sqrt{\cos^2 \theta + 2 i x}} = \int \frac{\alpha \sin \theta \frac{d \omega}{d x} d x}{\sqrt{\cos^2 \theta + 2 i x}}$$

$$= \frac{\alpha \sin \theta}{\sqrt{i}} \left\{ \left(\frac{d \omega}{d x}\right)'' z'' - \left(\frac{d \omega}{d x}\right)' z' \right.$$

$$- \frac{1}{3} \left\{ \left(\frac{d^2 \omega}{d x^2}\right)'' z''^3 - \left(\frac{d^2 \omega}{d x^2}\right)' z'^3 \right\} \quad [1]$$

$$+ \frac{1}{3 \cdot 5} \left\{ \left(\frac{d^3 \omega}{d x^3}\right)'' z''^5 - \left(\frac{d^3 \omega}{d x^3}\right)' z'^5 \right\} \Bigg\},$$

&c.

The second term is

$$- \frac{3 \alpha \sin \theta i^2 x^2 d \omega}{2 (\cos^2 \theta + 2 i x)^{\frac{3}{2}}}$$

$$= - \frac{3 \alpha \sin \theta}{8 i^{\frac{3}{2}}} \left\{ i^2 z^3 - 2 i z \cos^2 \theta + \frac{\cos^4 \theta}{z^2} \right\} \frac{d \omega}{d x} d \omega,$$

the integral of which is

$$- \frac{3 \alpha \sin \theta}{8 i^{\frac{3}{2}}} \left\{ \frac{d \omega}{d x} \left\{ \frac{i^2 z^3}{3} - 2 i z \cos^2 \theta - \frac{\cos^4 \theta}{z} \right\} \right.$$

$$\begin{aligned}
& -\frac{d^2 \omega}{d x^2} \left\{ \frac{i^2 z^5}{3 \cdot 5} - \frac{2 i z^3 \cos^2 \theta}{3} - z \cos^4 \theta \right\} \\
& + \frac{d^3 \omega}{d x^3} \left\{ \frac{i^2 z^7}{3 \cdot 5 \cdot 7} - \frac{2 i z^5}{3 \cdot 5} \cos^2 \theta - \frac{z^3}{3} \cos^4 \theta \right\}, \\
& \quad - \&c. \\
& = \frac{3 \alpha \sin \theta}{8 \sqrt{i}} \left\{ \frac{1}{z} \left\{ \frac{1}{i} \frac{d \omega}{d x} \cos^4 \theta \right\} \right. \\
& \quad + z \left\{ -\frac{1}{i} \frac{d^2 \omega}{d x^2} \cos^4 \theta + z \frac{d \omega}{d x} \cos^2 \theta \right\} \\
& \quad + \frac{z^3}{3} \left\{ \frac{1}{i} \frac{d^3 \omega}{d x^3} \cos^4 \theta + z \frac{d^2 \omega}{d x^2} \cos^2 \theta - i \frac{d \omega}{d x} \right\} \quad [2] \\
& \quad + \frac{z^5}{3 \cdot 5} \left\{ -\frac{1}{i} \frac{d^4 \omega}{d x^4} \cos^4 \theta + z \frac{d^3 \omega}{d x^3} \cos^2 \theta + i \frac{d^2 \omega}{d x^2} \right\} \\
& \quad \left. + \frac{z^7}{3 \cdot 5 \cdot 7} \left\{ \frac{1}{i} \frac{d^5 \omega}{d x^5} \cos^4 \theta - z \frac{d^4 \omega}{d x^4} \cos^2 \theta - i \frac{d^3 \omega}{d x^3} \right\} \right\}
\end{aligned}$$

In order to take this quantity between the proper limits, it is only necessary to write it first with two accents and then with one accent, and take the difference of the quantities so expressed.

Instead, however, of employing the preceding expressions, I shall now introduce the auxiliary quantity e employed by Mr. Ivory. Let

$$\tan \phi = \frac{\sqrt{2 i x''}}{\cos \theta} \quad e = \tan \frac{\phi}{2}$$

$$\tan \phi = \frac{2 e}{(1 - e^2)^2}$$

$$\sqrt{\cos^2 \theta + 2 i x} = \frac{\sqrt{2 i x''}}{2 e} \sqrt{(1 - e^2)^2 + \frac{4 e^2 x}{x''}}$$

I assume with Mr. Ivory

$$\sqrt{(1 - e^2)^2 + \frac{4 e^2 x}{x''}} = 1 - e^2 + 2 e^2 z$$

$$\frac{d x}{\sqrt{\cos^2 \theta + 2 i x}} = \frac{2 e x'' d z}{\sqrt{2 i x''}}$$

then $x = x'' z - x'' e^2 (z - z^2)$.

Suppose $d \omega$ contains any term of the form $A c^{-b x} d x$, then

$$\frac{\alpha \sin \theta d \omega}{\sqrt{\cos^2 \theta + 2 i x}} \quad \text{will contain the term}$$

$$\frac{2 \alpha \sin \theta A e c^{-x'' z} \times c^{x'' e^2 (z - z^2)} x'' dz}{\sqrt{2 i x''}},$$

which is to be integrated from $z = 0$ to $z = 1$. This integral may be exhibited under two aspects; in the first, which is that given by Mr. Ivory, the coefficients of the different powers of e consist of a finite number of terms. In other forms of the integral, which will be given here, applicable to all atmospheres of finite altitude, the coefficients are composed of an infinite number of terms, converging with rapidity and in a form suited for numerical computation.

$$c^{-b x'' z} = 1 - b x'' z + \frac{b^2 x''^2 z^2}{1.2} - \&c.$$

and the single term $A c^{-b x} dx$ in $d\omega$ will give in

$$\int \frac{\alpha (1 + \alpha) \sin \theta d\omega}{\sqrt{\cos^2 \theta + 2 i x}} \text{ the term}$$

$$\frac{2 A \alpha (1 + \alpha) \sin \theta}{\sqrt{2 i x''}} \left\{ \left\{ x'' - b x''^2 z + \frac{b^2 x''^2}{2} z^2 - \frac{b^3 x''^3}{2.3} z^3 + \&c. \right\} e dz \right.$$

$$+ b (1 - z) \left\{ x''^2 z - b x''^3 z^2 + \frac{b^2 x''^4}{2} z^3 - \frac{b^3 x''^5}{2.3} z^4 + \&c. \right\} e^3 dz$$

$$+ b^2 (1 - z)^2 \left\{ x''^3 z^2 - b x''^4 z^3 + \frac{b^2 x''^5}{2} z^4 - \frac{b^3 x''^6}{2.3} z^5 + \&c. \right\} e^5 dz$$

$$+ \&c. \left. \right\}$$

$$\int_0^1 z^m (1 - z)^n = \frac{n (n - 1) (n - 2) (n - 3) \dots 1}{(m + 1) (m + 2) \dots (m + n + 1)}.$$

Hence it will be seen that if $d\omega$ contains any number of terms of the form $A c^{-b x} dx$, the definite integral required

$$= \frac{2 (1 + \alpha) \alpha \sin \theta}{\sqrt{2 i x''}} \left\{ \left\{ x'' + \frac{x''^2}{2} \left(\frac{d\omega}{dx} \right)' + \frac{x''^3}{2.3} \left(\frac{d^2\omega}{dx^2} \right)' + \frac{x''^4}{2.3.4} \left(\frac{d^3\omega}{dx^3} \right)' + \&c. \right\} e \right.$$

$$+ \left\{ \frac{x''^2}{2.3} \left(\frac{d\omega}{dx} \right)' + \frac{x''^3}{3.4} \left(\frac{d^2\omega}{dx^2} \right)' + \frac{x''^4}{2.4.5} \left(\frac{d^3\omega}{dx^3} \right)' + \&c. \right\} e^3 \quad [1]$$

$$+ \left\{ \frac{x''^3}{3.4.5} \left(\frac{d^2\omega}{dx^2} \right)' + \frac{x''^4}{4.5.6} \left(\frac{d^3\omega}{dx^3} \right)' + \frac{x''^5}{2.5.6.7} \left(\frac{d^4\omega}{dx^4} \right)' + \&c. \right\} e^5$$

$$+ \&c. \left. \right\}$$

Another expression for $\delta\theta$ may be obtained in the following manner. Suppose $d\omega$ contains any term

$$A_n (x'' - x)^n dx = A_n X^n dx$$

Since

$$u - \frac{\alpha}{i} \omega = x \quad \frac{d x}{d u} = 1 - \frac{\alpha}{i} \frac{d \omega}{d u} = 1 - \frac{\alpha}{i} \omega_1$$

$$\frac{d \omega}{d u} = \frac{d \omega}{d x} \frac{d x}{d u}$$

therefore

$$\frac{d \omega}{d x} = \frac{\omega_1}{1 - \frac{\alpha}{i} \omega_1},$$

similarly

$$\frac{d^2 \omega}{d x^2} = \frac{\omega_2}{\left(1 - \frac{\alpha}{i} \omega_1\right)^3}$$

$$\frac{d^3 \omega}{d x^3} = \frac{\omega_3}{\left(1 - \frac{\alpha}{i} \omega_1\right)^4} + \frac{3 \frac{\alpha}{i} (\omega_2)^2}{\left(1 - \frac{\alpha}{i} \omega_1\right)^5}$$

$$\frac{d^4 \omega}{d x^4} = \frac{\omega_4}{\left(1 - \frac{\alpha}{i} \omega_1\right)^5} + \frac{10 \frac{\alpha}{i} \omega_3 \omega_2}{\left(1 - \frac{\alpha}{i} \omega_1\right)^6} + \frac{15 \frac{\alpha^2}{i^2} (\omega_2)^3}{\left(1 - \frac{\alpha}{i} \omega_1\right)^7}$$

The quantities $\frac{d \omega}{d X}$, $\frac{d^2 \omega}{d X^2}$, &c., might also be deduced from

$\frac{d \omega}{d U}$, $\frac{d^2 \omega}{d U^2}$, &c., by similar expressions, only changing the signs

of those terms which are multiplied by uneven powers of $\frac{\alpha}{i}$. I

have not, however, found it convenient to have recourse to this method of obtaining the development of ω in terms of X . I have employed the series

$$\omega = 1 - F X - \frac{\alpha}{2 i} \frac{d(F X)^2}{d X} - \frac{\alpha^2}{2 \cdot 3 i^2} \frac{d^2(F X)^3}{d X^2} - \&c.,$$

and I have found $\frac{\alpha}{2 i} (F X)^2$ by actual multiplication, $\frac{\alpha^2}{2 \cdot 3 i^2} (F X)^3$

by multiplying $\frac{\alpha}{2 i} (F X^2)$ by $\frac{\alpha}{2 i} (F X)$, &c. This process, though

somewhat tedious, is extremely easy. As it may be carried on systematically, and the numbers follow each other, it is not liable to error.

So far all is general; it now remains to make some supposition with regard to the function $f u$, upon which the constitution of the atmosphere depends. If we take, as in vol. xvi., p. 440, see also pres. vol. p. 279.

$$\omega = 1 - H^{\frac{1}{1-\gamma}} c^{-u} \left\{ c^{-u} - 1 + H \right\}^{\frac{1}{\gamma-1}}$$

$u = u'' - U$. See p. 490.

$$\omega = 1 - H^{\frac{1}{1-\gamma}} c^{-u''} c^U \left\{ c^{-u''} c^U - 1 + H \right\}^{\frac{1}{\gamma-1}}$$

$$c^{-u''} = 1 - H, \text{ therefore}$$

$$\omega = 1 - \frac{(1-H)^{\frac{\gamma}{\gamma-1}}}{H^{\frac{1}{\gamma-1}}} c^U \left\{ c^U - 1 \right\}^{\frac{1}{\gamma-1}}$$

If we take, as in p. 274, $\gamma = 1.5$,

$$1 - \omega = \frac{c^{-u}}{H^2} \left\{ c^{-u} - 1 + H \right\}^2 = (1-q)^2 (1-Hq)$$

$$f x = 1 - \frac{c^{-x}}{H^2} \left\{ c^{-x} - 1 + H \right\} \quad p = p' (1-q)^3$$

$$\omega = 1 - \frac{(1-H)^3}{H^2} \left\{ c^3 U - 2 c^2 U + c^U \right\},$$

$$i = \frac{k(1+\alpha\theta')}{a g H \beta}. \text{ See p. 280,} \quad \theta = \frac{g'}{p'} \left\{ \frac{1}{\alpha} + \theta' \right\} \frac{p}{g} - \frac{1}{\alpha}$$

$$\frac{E p^{\frac{\gamma-1}{\gamma}}}{1 - E p^{\frac{\gamma-1}{\gamma}}} = -H. \text{ See vol. xvi., p. 440.}$$

In page 470 I found $H = .54378$ (from the observations of M. Gay Lussac) corresponding to the temperature $87^{\circ}35$ of Fahrenheit, and to 30.145 inches of mercury in the barometer. As the uncertainty with respect to the values of γ and E appertaining to the mean state of the atmosphere makes it useless to have recourse to greater refinement, I shall now suppose that this value of H will be sufficiently exact for the temperature 50° of Fahrenheit and for 30 inches of mercury in the barometer at the earth's surface; the sequel will show that this hypothesis is admissible, and the calculation of i will stand thus: when $\gamma = 1.5$

$\log \frac{k}{g M} = 4.2633392$	$\log \beta = 9.5228787$
$\log M = 9.6377843$	$\log H = 9.7354232$
$\log (1 + \alpha \theta') = 0.0159881$	$a = 6.8041168$
3.9171116	6.0624187
6.0624187	

$$\log i = 7.8546929 \quad i = .0071564$$

$$u'' = \text{Nap. log } \frac{1}{1-H} = .78478.$$

The following table shows the constitution of the atmosphere with this system of constants. It should be recollected that in calculating this table, as well as those in p. 278 and p. 280, the law of Mariotte and Gay Lussac,

$$p = k \varrho (1 + \alpha \theta),$$

is implicitly supposed to hold good at very low temperatures, which is to a certain extent conjectural. For this reason, and for the reason that we have not at present sufficient data for determining with great precision the constants γ and E , it is not intended to attach precision to the temperatures, densities and pressures given in the following table for the altitudes beyond 5 miles. The following example will serve to show how the table was calculated :

Calculation of the Pressure, Temperature, and Density for the height of 10 miles.

$$\begin{array}{ll} \log 10 = 1.0000000 & i = 7.8546929 \\ \log a = 3.5974758 \text{ in miles} & \log M = 9.6377843 \end{array}$$

$$\begin{array}{r} 7.4025242 \\ \hline 8.2169086 \end{array}$$

$$\cdot 002526$$

$$7.4025242$$

$$\log 1.002526 = 0.0011364$$

$$7.4013878$$

$$8.2169086$$

$$9.1844792$$

$$\log Hq = 9.4725517$$

$$\log H = 9.7354232$$

$$9.7371285$$

$$\cdot 54592 = q$$

$$\cdot 45408 = 1 - q$$

$$\log (1 - q) = 9.6571324$$

$$2$$

$$9.6571324$$

$$3$$

$$1.2201080$$

$$\log p = 0.4485185$$

$$9.3142648$$

$$9.8470750$$

$$9.1613398$$

$$q = \cdot 14499$$

$$p = 2.81$$

$$8.9713972$$

$$1.4771213$$

$$0.4485185$$

$$1.6686265$$

$$\log \varrho = 9.1613398$$

$$2.5072867$$

$$321.6$$

$$448.0$$

$$\tau = [1.2201080] \frac{p}{\varrho} - 448^{\circ}$$

$$\tau = -126.4$$

Table showing the constitution of the Atmosphere.

Height in miles.	Pressure <i>p</i> .	Temp. <i>t</i> .	Density <i>ρ</i> .
0	Inch. 30.00	Fahr. +50.0	1.00000
1	24.61	35.0	.84611
2	20.07	19.5	.71294
3	16.25	+ 3.4	.59798
4	13.06	-13.3	.49903
5	10.41	30.6	.41403
10	2.81	126.4	.14499
15	.45	240.6	.03573
22.35	-448.0

According to this system of constants, the ascent for depressing Fahrenheit's thermometer 1° is about 352 feet.

$$\text{If } \log \alpha \text{ (in sex. sec.)} = 1.7669538 \quad \alpha^* = .00028348$$

$$x'' = u'' - \frac{\alpha}{i} = .74514 \quad \log \left\{ \frac{(1-H)^3}{H^2} \right\} = 9.5066765.$$

$$F U = \frac{(1-H)^3}{H^2} \left\{ c^3 U - 2c^2 U + c^U \right\}. \text{ See p. 473.}$$

$$\begin{aligned} &= [9.5066765] U^2 + [9.8077064] U^3 + [9.8254352] U^4 \\ &+ [9.6827677] U^5 + [9.4289405] U^6 + [9.0902531] U^7 \\ &+ [8.6829116] U^8 + [8.2178251] U^9 + [7.7028036] U^{10} \\ &+ [7.1436673] U^{11} + [6.5450488] U^{12} + [5.9104758] U^{13} \\ &+ [5.2429802] U^{14} + [4.5449962] U^{15} + [3.8186763] U^{16} + \&c. \end{aligned}$$

$F X$ is found by changing U into X in the above series. Although the development of ω might be obtained by procuring the quantities $\frac{d\omega}{dX}$, $\frac{d^2\omega}{dX^2}$, &c., from $\frac{d\omega}{dU}$, $\frac{d^2\omega}{dU^2}$, through the expressions given in p. 492. I have preferred employing the series

$$\begin{aligned} d\omega &= 1 - F X - \frac{\alpha}{2i} \frac{d(FX)^2}{dX} - \frac{\alpha^2}{2.3i^2} \frac{d^2(FX)^3}{dX^2} \\ \frac{d\omega}{dx} &= \frac{dFX}{dX} + \frac{\alpha}{2i} \frac{d^2(FX)^2}{dX^2} + \frac{\alpha^2}{2.3i^2} \frac{d^3(FX)^3}{dX^3} + \&c. \end{aligned}$$

By involution from the expression for $F X$ the following were obtained :

$$\begin{aligned} \frac{\alpha}{2i} (FX)^2 &= [7.3105250] X^4 + [7.9125849] X^5 + [8.2225672] X^6 \\ &+ [8.3653575] X^7 + [8.3901249] X^8 + [8.3259417] X^9 \end{aligned}$$

* I have adjusted the value of α so that the mean refraction at 45° might exactly agree with that of M. Bessel.

$$+ [8.1894155] X^{10} + [7.9925434] X^{11} + [7.7440827] X^{12} \\ + [7.4492548] X^{13} + [7.1140008] X^{14} + [6.7436508] X^{15} \\ + [6.3433889] X^{16} + [5.9048724] X^{17} + \&c.$$

$$\frac{\alpha^2}{2 \cdot 3 \cdot i^2} (F X^3) = [4.9382822] X^6 + [5.7162512] X^7 + [6.1990788] X^8 \\ + [6.5124208] X^9 + [6.7058125] X^{10} + [6.8073789] X^{11} \\ + [6.8341245] X^{12} + [6.7797954] X^{13} + [6.7124763] X^{14} \\ + [6.5779048] X^{15} + [6.3758464] X^{16} + [6.1424833] X^{17} \\ + [5.9091202] X^{18} + \&c.$$

$$\frac{\alpha^3}{2 \cdot 3 \cdot 4 \cdot i^3} (F X^4) = [2.4411007] X^8 + [3.3458338] X^9 + [3.9500667] X^{10} \\ + [4.3853642] X^{11} + [4.6996664] X^{12} + [4.9204991] X^{13} \\ + [5.0668412] X^{14} + [5.1470479] X^{15} + [5.1817987] X^{16} \\ + [5.1502344] X^{17} + [5.0587813] X^{18} + [4.9792296] X^{19} + \&c.$$

$$\frac{\alpha^4}{2 \cdot 3 \cdot 4 \cdot 5 \cdot i^4} (F X^5) = [9.8470092] X^{10} + [0.1480392] X^{11} + [1.5496223] X^{12} \\ + [2.0850470] X^{13} + [2.4878504] X^{14} + [2.8026972] X^{15} \\ + [3.0171128] X^{16} + [3.1689304] X^{17} + [3.2999429] X^{18} \\ + [3.3830969] X^{19} + [3.4680509] X^{20} + \&c.$$

The coefficients of the different powers of X in these series become very small, but they acquire large multipliers from the successive differentiations which are required to give the corresponding terms in the expression for ω .

I find with this constitution of the atmosphere, A_n being the coefficient of X^n in the expression for $\frac{d \omega}{d x}$.

$$A_1 = .6422 \\ A_2 = 1.9268 + .0245 = 1.9513 \\ A_3 = 2.6761 + .1635 + .0010 = 2.8406 \\ A_4 = 2.4085 + .5008 + .0109 + .0001 = 2.9203 \\ A_5 = 1.6110 + .9741 + .0531 + .0007 = 2.6389 \\ A_6 = .8617 + 1.3750 + .1640 + .0045 = 2.4052 \\ A_7 = .3854 + 1.5250 + .3657 + .0192 + .0003 = 2.2956 \\ A_8 = .1486 + 1.3920 + .6353 + .0595 + .0019 = 2.2373 \\ A_9 = .0504 + 1.0812 + .9009 + .1428 + .0074 \dots = 2.1827 \\ A_{10} = .0153 + .7322 + 1.0335 + .2802 + .0229 + .0007 \dots = 1.0848 \\ A_{11} = .0042 + .4389 + 1.1265 + .4596 + .0561 + .0029 \dots = 2.6882 \\ A_{12} = .0010 + .2366 + 1.0329 + .6852 + .1094 + .0090 \dots = 2.0741 \\ A_{13} = .0002 + .1163 + .8410 + .8072 + .2051 + .0226 \dots = 1.9924 \\ A_{14} = \dots + .0529 + .5644 + .8410 + .3371 + .0492 \dots = 1.8466. \\ \frac{d \omega}{d x} = .6422 X + 1.9513 X^2 + 2.8406 X^3 + 2.9203 X^4 \\ + 2.6389 X^5 + 2.4052 X^6 + \&c.$$

Hence by substituting these values of A_1 , A_2 , &c., in the expression for $\delta \theta$ given in p. 491, I find the first term in the refraction

$$= \sin \theta \{ 1132'' \cdot 8 e + 639'' \cdot 9 e^3 + 220'' \cdot 4 e^5 \\ + 60'' \cdot 5 e^7 + 17'' \cdot 8 e^9 + 5'' \cdot 5 e^{11} + \&c. \} \quad [1]$$

At the horizon $e = 1$, and this portion of the horizontal refraction = $2076'' \cdot 9$.

The second term in the refraction is

$$= - \frac{3}{2} \frac{\sin \theta \alpha i^2 x^2 d\omega}{(\cos^2 \theta + 2ix)^{\frac{5}{2}}} \\ = - \frac{3 \cdot 4 \sqrt{i} \alpha \sin \theta x''^2 z^2 \{1 - e^2 + e^2 z\}^2 e^3 \frac{d\omega}{dx} dz}{2 \sqrt{2} x'' \{1 - e^2 + 2e^2 z\}^2} \\ = - \frac{3 \cdot 4 \sqrt{i} \alpha \sin \theta x''^2 z^2 e^3 \frac{d\omega}{dx} dz}{2 \sqrt{2} x''} \left\{ 1 - 2ze^2 \right. \\ \left. + \{5z^2 - 2z\} e^4 - \&c. \right\}$$

Suppose $d\omega$ contains any term

$$A_n (x'' - x)^n dx \\ x'' - x = x'' (1 - z) (1 + e^2 z)$$

$$d \cdot \delta \theta = - \frac{3 \cdot 4 \sqrt{i} \alpha \sin \theta x''^{n+2}}{2 \sqrt{2} x''} A_n (1 - z)^n (1 + e^2 z)^n z^2 e^3 dz \\ \left\{ 1 - 2ze^2 + \{5z^2 - 2z\} e^4 - \&c. \right\}$$

Neglecting the higher powers of e

$$\theta = - \frac{3 \cdot 4}{2} \frac{\sqrt{i} \alpha \sin \theta e^3}{\sqrt{2} x''} \left\{ \frac{2 \cdot 1 A_1 x''^3}{2 \cdot 3 \cdot 4} + \frac{2 \cdot 1 A_2 x''^4}{3 \cdot 4 \cdot 5} + \frac{2 \cdot 1 A_3 x''^5}{4 \cdot 5 \cdot 6} \right. \\ \left. + \&c. \right\} \quad [2]$$

With the same constants as before, $\gamma = 1 \cdot 5$, $H = \cdot 54378$

$$\delta \theta = - [1 \cdot 3861838] \sin \theta e^3 \left\{ \frac{2 \cdot 1 A_1 x''^3}{2 \cdot 3 \cdot 4} + \frac{2 \cdot 1 A_2 x''^4}{3 \cdot 4 \cdot 5} + \frac{2 \cdot 1 A_3 x''^5}{4 \cdot 5 \cdot 6} \right. \\ \left. + \&c. \right\} \quad [2] \\ = - 1'' \cdot 5 \sin \theta e^3.$$

This term thus amounts to only $1'' \cdot 5$ at the horizon; according to Mr. Ivory it does not amount to more than $1''$.

Hence, finally, the refraction is expressed by the following series:—

$$\text{Ref.} = \sin \theta \{ 1132'' \cdot 8 e + 638'' \cdot 4 e^3 + 220'' \cdot 4 e^5 \\ + 60'' \cdot 5 e^7 + 17'' \cdot 8 e^9 + 5'' \cdot 5 e^{11} + \&c. \}$$

$$= \sin \theta \{ [3.0541728] e + [2.8051475] e^3 \\ + [2.3443834] e^5 + [1.7821564] e^7 \\ + [1.2501754] e^9 + [0.7409070] e^{11} + \&c. \}$$

$$\tan \phi = \frac{[9.0139814]}{\cos \theta} \quad e = \tan \frac{\phi}{2}$$

Mr. Russell has calculated a table of refractions from the above formula; and the following comparative view has been drawn up, with Bessel's table in the *Tabulæ Regiomontanæ* (which may be considered as the result of observations), with the table published annually in the *Conn. des Temps*, and with Mr. Ivory's table, recently published in the *Phil. Trans.* 1838, p. 224.

Tables of Mean Refractions.

Bar. 30 inch. Therm., Fahr., 50°.

App. Zenith Dist.	Mean Refractions.				App. Zenith Dist.
	Calculated.			Observed.	
	Conn. des Temps.	Ivory.	New Table*.	Tab. Reg.	
10	10''30	10''30	10''30	10''30	10
20	21.26	21.26	21.26	21.26	20
30	33.72	33.72	33.72	33.72	30
40	48.99	48.99	48.99	48.99	40
45	58.36	58.36	58.36	58.36	45
50	69.52	69.52	69.51	69.52	50
55	83.25	83.25	83.24	83.24	55
60	100.86	100.85	100.85	100.85	60
65	124.65	124.65	124.63	124.62	65
70	159.22	159.16	159.16	159.11	70
75	214.83	214.70	214.68	214.58	75
80	320.63	320.19	320.08	319.88	80
81	354.33	353.79	353.64	353.38	81
82	395.37	394.68	394.47	394.20	82
83	445.87	445.42	445.11	444.86	83
84	511.22	509.86	509.34	509.23	84
85	595.80	593.96	593.13	593.38	85
85½	648.34	646.21	645.15	647.10	85½
86	710.07	707.43	706.04	707.15	86
86½	783.07	779.92	778.08	777.36	86½
87	870.37	866.76	864.30	864.59	87
87½	975.89	971.93	968.84	972.21	87½
88	1105.1	1101.35	1097.26	1101.40	88
88½	1265.0	1262.6	1257.66	1265.5	88½
89	1464.9	1466.8	1461.49	1481.8	89
89½	1716.4	1729.5	1725.70	1764.9	89½
90		2072.6	2075.4		90

* The constitution of the atmosphere is shown by the table in p. 498.

The following table shows the errors of the table of the *Conn. des Temps*, of Mr. Ivory's table, and of my table, assuming Bessel's to be correct.

Zenith Dist.	Error of Table of Conn. des Temps.	Error of Mr. Ivory's Table.	Error of New Table.	Zenith Dist.	Error of Table of Conn. des Temps.	Error of Mr. Ivory's Table.	Error of New Table.
70	+ "11	+ "05	+ "05	86	+ 3'12	+ "28	- "11
75	+ .25	+ .12	+ .10	86½	+ 5'71	+ 2'56	+ .72
80	+ .75	+ .31	+ .20	87	+ 5'78	+ 2'17	- .29
81	+ .95	+ .41	+ .26	87½	+ 3'68	- .28	- 3'37
82	+ 1'17	+ .48	+ .27	88	+ 3'70	- .05	- 4'14
83	+ 1'01	+ .56	+ .25	88½	- 0'50	- 2'90	- 7'80
84	+ 1'99	+ .63	+ .11	89	- 16'90	- 15'00	- 20'30
85	+ 2'42	+ .58	- .25	89½	- 48'50	- 35'40	- 39'20
85½	+ 1'24	- .89	- 1'95				

I think that the discrepancies about $85\frac{1}{2}$, 86, $86\frac{1}{2}$, are caused by irregularities in the refractions of the *Tab. Reg.* Groombridge, who made many observations for the purpose of determining the amount of the refraction near the horizon, makes the horizontal refraction, for barometer 30 inch, and therm. Fahr. 50° , $2075''\cdot4$.* There is, however, some uncertainty respecting this quantity, and generally respecting the amount of refractions near the horizon. Upon this point see Delambre, *Ast.*, vol. i. p. 319. Mr. Ivory says "There is great probability that the horizontal refraction is very near $2070''$, and does not exceed that quantity."

But for the irregularity in Bessel's table, which is clearly seen in the diagram inserted in the annexed plate, my table of mean refractions would be identical with the table of that distinguished astronomer to within 3 degrees of the horizon. It may therefore be safely concluded that the refractions, which belong to the atmosphere, constituted as I have supposed, in conformity with my theory of the heat of steam and other vapours, are consistent with observation.

The quantities denoted by u , (g), s in the *Mécanique Céleste* correspond to the quantities, i x , g' , and $\frac{z}{a}$ of this treatise. The equation

$$s - \alpha \left[1 - \frac{g}{(g)} \right] = u, \text{ Méc. Cé., vol. iv. p. 262,}$$

corresponds to the equation

$$u - \frac{\alpha}{i} \omega = x \text{ of p. 498.}$$

* This curious coincidence, with my value of the horizontal refraction, is of course partly accidental.

Laplace assumes the relation between ω and x .

$$\varrho = (\varrho) \left[1 + \frac{f u}{l'} \right] c^{-\frac{u}{l'}}$$

or in the notation of this treatise

$$\varrho = \varrho' \left\{ 1 + \frac{i f}{l'} x \right\} c^{-\frac{i x}{l'}}$$

$$\omega = -\frac{i f}{l'} x c^{-\frac{i x}{l'}}$$

f and l' being arbitrary quantities, such that

$$f = \cdot 49042 \quad l' = \cdot 000741816.$$

A table, similar to that which I have given in p. 498, showing the constitution of the atmosphere, which Laplace has assumed, would be instructive, and would enable us to judge of the admissibility of the conditions attributed to the higher regions of the atmosphere by that great philosopher.

In this treatise I have obtained an expression* for the altitude in terms of the pressure, founded upon the conditions of elastic vapours generally; this gives the relation between u and ω (see p. 471) from which a relation between x and u must afterwards be sought. When on the contrary the relation between ω and x is assumed (as was done by Laplace) an advantage may be gained in the calculation of the refraction, at the expense, however, of a simple and intelligible definition of the constitution of the atmosphere; and such a relation is of course also unconnected with any considerations founded upon the nature of caloric.

Mr. Ivory assumes the relation

$$\frac{p}{p'} = \frac{7}{9} \frac{\varrho}{\varrho'} + \frac{2}{9} \frac{\varrho^2}{\varrho'^2}$$

p' denoting the pressure, and ϱ' the density of the atmosphere at the earth's surface. From this relation it follows that (see p. 473)

$$a i u = \frac{7 k (1 + \alpha \vartheta')}{9 g} \log \frac{\varrho'}{\varrho} + \frac{4 k (1 + \alpha \vartheta')}{9 g} \left(1 - \frac{\varrho}{\varrho'} \right)$$

k, α, ϑ' are L, β, τ' , in Mr. Ivory's notation.

* $\frac{z}{1 + \frac{z}{a}} = -a i \log (1 - H q).$

When u is a simple function of ω , this value of u may be substituted in the equation

$$x = u - \frac{\alpha}{i} \omega^*, \quad (\text{p. 489})$$

and the value of ω in terms of x may be found at once by the reversion of the series.

Mr. Ivory makes $\frac{k(1 + \alpha \theta')}{a g} = i$, so that

$$\begin{aligned} x &= -\log(1 - \omega) + f \log(1 - \omega) + \left\{ 2f - \frac{\alpha}{i} \right\} \omega \\ &= -\log(1 - \omega) + f \log(1 - \omega) + h \omega. \end{aligned}$$

This equation corresponds to the equation of Mr. Ivory

$$x = u - \lambda(1 - c^{-u}) - f \frac{d c^{-u} R_2}{c^{-u} d u} - f' \frac{d^2 c^{-u} R_4}{c^{-u} d u^2} - \&c.$$

p. 203, when $f' = 0$. $R_2 = 1 - u - c^{-u}$ $\omega = 1 - c^{-u}$.

The table of mean refractions given by Mr. Ivory is founded upon the supposition that $f', f'', \&c. = 0$.

$$\text{Let } i' x' = -\frac{k(1 + \alpha \theta)(1 - f)}{a g} \log(1 - \omega)$$

$$+ \left\{ \frac{2k(1 + \alpha \theta)}{a g} f - \alpha \right\} \omega$$

$$\text{and let } i' = \frac{k(1 + \alpha \theta)(1 - f)}{a g} = (1 - f) i$$

$$h' = \frac{h}{1 - f} \quad \lambda = \frac{\alpha}{i} \quad h = 2f - \lambda$$

$$x' = -\log(1 - \omega) + h' \omega$$

i and h are identical with the quantities represented by those letters by Mr. Ivory, if

$$\alpha = \cdot 0002835, \quad i = \cdot 0012958, \quad h = \cdot 22566, \quad f = \frac{2}{9}$$

$$\text{then} \quad i' = \cdot 0010078, \quad h' = \cdot 29012.$$

By Lagrange's theorem I find

* Mr. Ivory has the equivalent equation $\frac{\sigma}{a} = \frac{s}{a} + \alpha \omega = i x + \alpha \omega$, p. 203. Mr. Ivory's σ is $a i u$ in the notation of this treatise.

$$\omega = 1 - c^{h'} c^{-x'} + h' c^{2h'} c^{-2x'} - \frac{3 h'^2}{1.2} c^{3h'} c^{-3x'} \\ + \frac{4^3 h'^3}{1.2.3} c^{4h'} c^{-4x'} - \&c.$$

$$\frac{d\omega}{dx'} = c^{h'} c^{-x'} - 2 h' c^{2h'} c^{-2x'} + \frac{3^3 h'^3}{1.2} c^{3h'} c^{-3x'} \\ + \frac{4^3 h'^3}{1.2.3} c^{4h'} c^{-4x'} - \&c.$$

The first part of the refraction is given by the expression

$$\alpha (1 + \alpha) \sin \theta \int_0^\infty \frac{\frac{d\omega}{dx'} dx'}{\sqrt{\cos^2 \theta + 2 i' x'}}$$

Let

$$n \left\{ \frac{\cos^2 \theta}{2 i'} + x' \right\} = z^2$$

$$n x' = z^2 - \frac{n \cos^2 \theta}{2 i'} \quad dx = \frac{2 z dz}{n}$$

$$\int_0^\infty \frac{c^{-n x'} dx'}{\sqrt{\cos^2 \theta + 2 i' x'}} = \frac{2 c^{z'}}{\sqrt{2 i'} \sqrt{n}} \int_{z'}^\infty c^{-z^2} dz$$

At the horizon $\cos \theta = 0$, $z' = 0$

$$\int_0^\infty \frac{c^{-n x'} dx'}{\sqrt{2 i' x'}} = \frac{2}{\sqrt{2 i'} \sqrt{n}} \int_0^\infty c^{-z^2} dz = \frac{\sqrt{\pi}}{\sqrt{2 i'} \sqrt{n}}$$

This part of the horizontal refraction

$$= \frac{\alpha (1 + \alpha) \sqrt{\pi}}{\sqrt{2 i'}} \left\{ c^{h'} - \frac{2 h' c^{2h'}}{\sqrt{2}} + \frac{3^2 h'^2 c^{3h'}}{1.2 \sqrt{3}} - \frac{4^3 h'^3 c^{4h'}}{1.2.3 \sqrt{4}} + \&c. \right\}$$

$$= \frac{\alpha (1 + \alpha) \sqrt{\pi}}{\sqrt{2 i'}} \left\{ 1 + \frac{1}{2} f + h - \frac{2 h}{\sqrt{2}} \right. \\ + \frac{1.3}{2.4} f^2 + \frac{3}{2} h f + \frac{h^2}{1.2} - \frac{2.3}{\sqrt{2} 2} h f - \frac{2^2 h^2}{\sqrt{2}} + \frac{3^2 h^2}{\sqrt{3} 1.2} \\ + \frac{1.3.5}{2.4.6} f^3 + \frac{3.5}{2.4} h f^2 + \frac{5}{1.2.2} h^2 f + \frac{h^3}{1.2.3} \\ - \frac{2.3.5}{\sqrt{2} 2.4} h f^2 - \frac{2^2.5}{\sqrt{2} 2} h^2 f - \frac{2^3 h^3}{\sqrt{2} 1.2} \\ \left. + \frac{3^2.5}{\sqrt{3} 2.2} h^2 f + \frac{3^3}{1.2 \sqrt{3}} h^3 - \frac{4^3}{1.2.3 \sqrt{4}} h^3 \right\}$$

$$\begin{aligned}
& + \frac{1.3.5.7}{2.4.6.8} f^4 + \frac{3.5.7}{2.4.6} h f^3 + \frac{5.7}{1.2.2.4} h^2 f^2 + \frac{7 h^3 f}{1.2.3.2} + \frac{h^4}{1.2.3.4} \\
& - \frac{2.3.5.7}{\sqrt{2} 2.4.6} h f^3 - \frac{2^2.5.7}{\sqrt{2} 2.4} h^2 f^2 - \frac{2^3.7}{\sqrt{2} 1.2.2} h^3 f \\
& - \frac{2^4 h^4}{\sqrt{2} 1.2.3} + \frac{3^2.5.7}{1.2 \sqrt{3} 2.4} h^2 f^2 + \frac{3^3.7}{1.2 \sqrt{3} 2} h^3 f + \frac{3^4 h^4}{1.2 \sqrt{3} 1.2} \\
& - \frac{4^3 7}{1.2.3 \sqrt{4} 2} h^3 f - \frac{4^4}{1.2.3.4 \sqrt{4}} h^4 + \frac{4^5}{1.2.3.4.5 \sqrt{5}} h^4 + \&c. \\
& = \frac{\alpha (1 + \alpha) \sqrt{\pi}}{\sqrt{2} i} \left\{ 1 + \lambda (\sqrt{2} - 1) - f \left(2 \sqrt{2} - \frac{5}{2} \right) \right. \\
& \quad \left. + h^2 \left\{ \frac{1}{2} - 2 \sqrt{2} + \frac{3}{2} \sqrt{3} \right\} - \frac{3}{2} h f (\sqrt{2} - 1) + \frac{3}{8} f^2 \right\}
\end{aligned}$$

when the higher powers of f and h are rejected, and this expression agrees with that given by Mr. Ivory, *Phil. Trans.*, 1838, p. 207.

$$\frac{\alpha (1 + \alpha) \sqrt{\pi}}{\sqrt{2} i} = 2036'' \cdot 5$$

In atmospheres which extend to an infinite distance m (or x'' in the notation of this treatise) is infinite and e always = 1, so that in this case the method employed by Mr. Ivory in p. 211 of his memoir, *Phil. Trans.*, 1838, would seem at least to require further elucidation. Mr. Ivory has avoided this consideration, which would otherwise arise with the atmosphere which he has assumed, by imposing an arbitrary limit to the altitude of his atmosphere, while, however, if I am not mistaken, upon his own assumption, the density and the pressure are still finite. When n is large the numerators of the separate quantities of which the quantity A_{2n+1} in p. 211 is composed become large also.

I do not find in Mr. Ivory's paper any remarks tending to prove that the quantities which he has discarded depending upon the higher powers of f and h are incapable of producing any sensible effect; taken separately they are by no means insignificant. Nor do I think it follows as a matter of course, even if the positive and negative terms are numerically of equal value at the horizon, and so fortunately cut one another out, that the same thing will happen necessarily at all other altitudes. Unless the approximation is pushed so far as to secure the retention of all the sensible terms, or those which fairly come within the limits of the errors of observation, any comparison of the result with the valuable table of M. Bessel is illusory and only calculated to lead to incorrect conclu-

sions. It is also indispensable that the relation implied or expressed between z and ω should be in exact conformity with the conditions attributed to the atmosphere, and in this respect the table of mean refractions of the late Mr. Atkinson in the Memoirs of the Astronomical Society appears to me not to rest upon a solid foundation.

Mr. Ivory connects the pressure and the density by the relation

$$\frac{p}{p'} = \cdot 77777 \frac{g}{g'} + \cdot 22222 \frac{g^2}{g'^2}.$$

M. Biot finds

$$\begin{aligned} \frac{p}{p'} &= \cdot 761909002718 \frac{g}{g'} + \cdot 238167190564 \frac{g^2}{g'^2} \\ &\quad - \cdot 000076193282, \end{aligned}$$

when the coefficients are so taken as to apply as nearly as the question will admit of throughout the whole extent of the atmosphere. But, by a careful examination of the data, M. Biot finds that at the earth's surface the following relation is more accurate.

$$\begin{aligned} \frac{p}{p'} &= \cdot 956643870584 \frac{g}{g'} + \cdot 120146052460 \frac{g^2}{g'^2} \\ &\quad - \cdot 076789923044 \quad (\text{p. 69.}) \end{aligned}$$

and at the upper limit of the atmosphere

$$\begin{aligned} \frac{p}{p'} &= \cdot 6604978157646 \frac{p}{p'} + \cdot 4159581823536 \frac{p^2}{p'^2} \\ &\quad - \cdot 00006605394115. \end{aligned}$$

According to my view this equation does not contain the true mathematical law which connects the density and pressure, but of course a parabola of this kind may always be found which will osculate the true curve at any given point.

In the *Comptes Rendus des Séances de l'Académie des Sciences*, tom. viii. p. 95, M. Biot verified and adopted a calculation of Lambert, who found from the phenomena of twilight the altitude of the atmosphere (*hauteur des dernières particules d'air réfléchissantes*) to be 29,115 metres.

It is unnecessary to dwell any further at present upon this subject, because if my theory of the Heat of Vapours be correct, the calculation of Astronomical Refractions, founded upon conditions which are not in conformity with that theory, becomes a problem of mere curiosity.

LXXV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

*Anniversary Address of the Rev. Prof. Buckland, President,
Feb. 21, 1840.*

[Continued from p. 396, and concluded.]

POSITIVE GEOLOGY.—DEVONIAN SYSTEM.

IN the Home Department of Positive Geology, the most striking circumstance has been an announcement by Professor Sedgwick and Mr. Murchison of the conclusion to which they were led by Mr. Lonsdale's suggestion in December 1837, founded on the intermediate character of the fossils in the Plymouth and Torbay limestone—that the greater part of the slate rocks of the south of Devon and of Cornwall belong to the old red sandstone formation.

The order of the observations which have led to this important result, is nearly as follows:—

In a paper read at Cambridge, during the winter of 1836–37, Professor Sedgwick considered the fossiliferous slates on both sides of Cornwall to be of the same formation, and coeval, or nearly so, with the calcareous rocks that lie between the slates of South Devon.

In 1836 and 1837 also*, Messrs. Sedgwick and Murchison proposed to transfer the culmiferous or anthracitic shale and grits (Shillot and Dunstone) of *North Devon* to the carboniferous system; withdrawing them from the grauwacke in which they had before been included, and thus assigning a much more recent date than heretofore to the strata which occupy nearly one third part of the map of Devonshire.

But the relations of the slates and limestones of *South Devon* still remained to be determined; the mineral characters of the former being different from those of the old red sandstone beneath the carboniferous group, in many parts of South Wales and in Herefordshire, while the true position of the limestones (e. g. those of Plymouth, Torbay, and Newton Bushell,) was doubtful. At this period (1837), the fossils of this district were examined by Mr. Lonsdale and Mr. Sowerby, to whom the organic remains, both of the carboniferous and Silurian systems, were familiar. It was soon perceived, that while some of the South Devonshire fossils approached to those of the carboniferous strata, and others to those of Siluria, there were still many species which could not be assigned to either system; the whole, taken together, exhibiting a peculiar and intermediate palæontological character. Mr. Lonsdale therefore suggested, that the difficulties which had perplexed this inquiry could be removed by regarding the limestones of South Devon as subordinate to slaty rocks, which represent the old red sandstones of Here-

* In August 1836, at the Meeting of the British Association at Bristol; and in a paper read before the Geological Society, May and June, 1837. now published in the Geological Transactions, Second Series, vol. v., Part 3,

ford, Wales, Scotland, and Ireland,—their true place in the series of Devonshire being intermediate between the culmiferous basin of North Devon, and the Silurian strata,—if the latter exist in that county.

The value of this suggestion was not at first appreciated; but after the lapse of more than a year, Mr. Lonsdale's views were adopted (March 1839) by Messrs. Sedgwick and Murchison*, who soon afterwards applied this new arrangement not only to the groups of Devonshire originally under review, but with a boldness which does credit to their sagacity, extended it to the whole of the slaty and calciferous strata of *Cornwall*, till then known only as grauwacke, clay-slate, or killas; assigning to those strata, likewise, the date of the old red sandstone, and resting this determination entirely on the character of the fossils. This change—the greatest ever made at one time in the classification of our English formations—was announced in a memoir read before the Geological Society in April 1839†; the authors then also proposing for the whole series (including both the old red sandstones of Herefordshire, and the fossiliferous slates and limestones of South Devon and Cornwall,) the new name of "*the Devonian system*," and expressing their belief, that many of the groups hitherto called grauwacke, in other parts of the British Islands and on the continent, would ere long be referred to the same geological epoch.

The proposed alteration, therefore, will terminate the perplexity hitherto arising from the circumstance, that the *old* red sandstone of Werner has been frequently confounded with the *new* red sandstone formation of English geologists. It also explains the cause of the English old red sandstone having been rarely recognised on the continent:—for if the Devonian slates afford the normal type of this formation, whilst the marly sandstones and conglomerates of Herefordshire are abnormal exceptions in it, we see the reason why their slaty continental equivalents, like the greater part of the South Devon slates, have been referred to the undivided Wernerian formation of grauwacke.

* It is to be observed here, that Mr. Murchison, having previously shown that the fossils of the Silurian æra are distinct from those of the carboniferous period, had also pointed out "the vast accumulations" (in which few fossils had at that time been discovered) "then known to separate the two systems." He mentions especially, that "the *fishes* of the old red sandstone—entirely distinct as to form and species—are as unlike those of the Silurian system, as they are to those of the overlying carboniferous system:" adding, "that he has no doubt, although at present unprovided with geological links to connect the whole series, that such proofs will be hereafter discovered, and that we shall then see in them as perfect evidence of a transition between the old red sandstone and carboniferous rocks, as we now trace from the Cambrian, through the Silurian, into the old red system."—See *Silurian System*, p. 585, line 22, *et seq.*

† [Abstracts of this and all the other papers referred to in this Address as having been read before the Geological Society during the year preceding its delivery, will be found in *L. & E. Phil. Mag.* vol. xiv. xv. xvi. and in the present volume.—EDIT.]

Mr. Austen, in a communication relating to the structure of the south of Devon, has identified the calcareous slate and limestone of the south of Cornwall with the limestones of this district, and considers that of Torbay among the newest deposits in the latter series.

The Rev. D. Williams also has communicated two papers respecting these disputed rocks, which he refers to the transition or grauwacke system, and endeavours to show that the strata of Devonshire can be distinguished into certain groups by their lithological characters.

Mr. De la Beche in his map of Devon and Cornwall, published in 1839, has adopted divisions of the strata, similar to those of Professor Sedgwick and Mr. Murchison, as to their order of sequence; applying, provisionally, to the culmiferous rocks the name of *Carbonaceous series*, and to the Devonian and Cornish slates the appellation of *Greywacke*.

We know also on the authority of Mr. De la Beche that tin mines are worked in carbonaceous rocks at Owlescomb near Ashburton, on the east side of the Dartmoor granite, and on its west side at Wheal Jewel near Tavistock. He further informs us that one of the richest tin mines now worked in Cornwall, namely the Charles-town mine, east of St. Austle, is in a fossiliferous rock containing *Enerinites* and corals, and that the same corals occur also near tin mines at St. Just; and in the neighbourhood of Liskeard the Rev. D. Williams has found slates which contain vegetable impressions, dipping under other slates which are intersected by lodes of tin and copper.

From these new facts, we learn that the killas and other slate rocks of Cornwall and the south of Devon do not possess the high antiquity which has till lately been imputed to them; and that tin occurs, as copper, lead and silver have long been known to do, not only in slate rocks that contain organic remains, but even in the coal formation.

Soon after the publication of the views of Messrs. Sedgwick and Murchison, a similar change was applied by Mr. Griffith to the south-west portion of his geological map of Ireland. In a paper that accompanied the presentation of this map to us on 22nd of May last, he states that he has now coloured, as old red sandstone and carboniferous limestone, extensive districts of the counties of Kerry, Cork, and Waterford, previously considered of higher antiquity; imputing his former erroneous opinion to the identity in lithological character of the shales and grits of the old red sandstone and carboniferous systems, with the older rocks in the transition series.

Mr. Griffith has also demonstrated by sections the unconformable position of the carboniferous and old red sandstone formations, which overlie older and more highly inclined slates in the counties of Kerry, Cork, Waterford, and Wexford.

Mr. Charles William Hamilton has likewise adopted similar changes; and believes that the slates which occupy a large space between the Mourne Mountains and Dublin are equivalent to those near Cork, which he now transfers to the old red sandstone.

Mr. Greenough, in the new edition of his map of England, represents nearly the same boundaries and order of succession in Devon and Cornwall as we find in the maps of Mr. De la Beche and Messrs. Sedgwick and Murchison; but in his memoir connected with the map, adopting the name of *Carbonaceous series* for the culmiferous rocks, he substitutes that of *Upper killas* for the Devonian system of Sedgwick and Murchison, (including under that term the old red sandstone of Herefordshire,) and *Lower killas* for the slates inferior to the Silurian system, which they have termed Cambrian.

Mr. Greenough, in his memoir, also shows by quotations from Dr. MacCulloch, that the undisputed old red sandstone of the north of Scotland exhibits, at intervals, the same great changes of mineral character, that occur in the strata intermediate between the Carbonaceous and Silurian systems in the west of England and on the borders of Wales; and justly infers the inadequacy of any one term to characterize formations which vary so much in lithological composition, that at one place they present the condition of a fine-grained silky slate, at another of sandstone, and at a third that of coarse gravel and conglomerate rock.

Thus, with respect to the slate rocks of Devon, Cornwall and Wales, the difficulties are reduced to those of an unsettled nomenclature; whilst nearly all parties are in unison as to the fundamental fact of referring the slates of South Devon and Cornwall to the epoch of the old red sandstone formation. The term *grauwacke*, however, I rejoice to think, will not be condemned to the extirpation which has been threatened from the nomenclature of geology; it may still retain its place as a generic appellative, comprehending the entire transition series of the school of Freyberg, and divisible into three great subordinate formations:—the Devonian system of Sedgwick and Murchison being equivalent to the upper *grauwacke*, the Silurian to the middle *grauwacke*, and the Cambrian system to the lower.

In this threefold distribution of the vast series of strata which have hitherto been indiscriminately designated by the common term *grauwacke*, we are, as it were, extending the progressive operations of a general inclosure act over the great common field of geology; we propose a division, founded on measurements, surveys, and the study of organic remains, analogous to that of the secondary strata, from the chalk downwards to the coal formation, established by William Smith, and to the separations of the once undivided territory of the great tertiary system, effected by Cuvier and Brongniart, Desnoyers, Lyell, and Deshayes.

To the uninitiated in geology, rectifications in the distribution of strata upon so large a scale may seem calculated to shake confidence in all the conclusions of our science; but a contrary inference will be drawn by those who know that these corrections have never been applied to conclusions established on the sure foundation of organic remains, but to those rocks only of which the arrangement had been founded on the uncertain character of mineral composition.

COAL FORMATION.

The Society has received from Professor Ansted a paper on the Carboniferous and Transition Rocks of Bohemia, a country which he visited last summer, directing especial attention to the district between Prague, Luditz and Pilsen, which he has illustrated by sections made from personal observation. Above the fundamental granite and gneiss he found extensive deposits of grauwacke, on which lie, in unconformable superposition, disconnected patches of the coal formation. The age of this coal is well known, from the fossil Flora of Count Sternberg, who resided in the midst of it near Swina, to be identical with that of the great Coal formation of England. Mr. Ansted gives information also as to the action of trap rocks in producing disturbances of the strata in this district; and respecting dislocations, by which the grauwacke is several times placed on a level with the coal measures, whilst in some cases the strata are inverted and the coal measures laid beneath the grauwacke.

We have received an interesting communication from Mr. Hawkshaw respecting a remarkable disclosure made in the Bolton Railway, six miles north of Manchester, of five fossil trees in a position vertical to the plane of the strata in which they stand. The roots are imbedded in a soft argillaceous shale immediately under a thin bed of coal. Near the base of one tree, and beneath the coal, more than a bushel of hard clay nodules was found, each inclosing a cone of *Lepidostrobus variabilis*. The bark of the trees was converted to coal, from one quarter to three quarters of an inch thick; the substance which has replaced the interior of the trees is shale; the circumference of the largest of them is $15\frac{1}{2}$ feet at the base, $7\frac{1}{2}$ at the top, and its height 11 feet. One tree has spreading roots, four feet in circumference, solid and strong. By the care of Mr. Hawkshaw these trees have been preserved, and a covering is erected over them. The attendant phenomena seem to show that they grew upon the strata that lie immediately beneath their roots*.

Mr. Barber Beaumont, in a communication respecting these same trees, considers that no drifted plants occur in coal fields, and that all the vegetables which are now converted into coal, grew upon swampy islands covered with luxuriant vegetation, which accumulated in the manner of peat bogs; that these islands, having sunk beneath the sea, were there covered with sand, clay and shells, till they again became dry land, and that this operation was repeated in the formation of each bed of coal. In denying altogether the presence of drifted plants, the opinion of the author seems erroneous; universal negative propositions are in all cases dangerous, and more especially so in geology: that some of the trees which are found erect in the coal formation have not been drifted, is, I think, established on sufficient evidence; but there is equal evidence to show that other trees, and leaves innumerable which pervade the strata that alternate with the coal, have been removed by water to con

[* See the abstract of this paper in L. & E. Phil. Mag. vol. xv. p. 539, and also that of a further communication from Mr. Hawkshaw, in the present number.—EDIT.]

siderable distances from the spots on which they grew. Proofs are daily increasing in favour of both opinions: viz. that some of the vegetables which formed our beds of coal grew on the identical banks of sand and silt and mud, which being now indurated to stone and shale, form the strata that accompany the coal; whilst other portions of these plants have been drifted, to various distances, from the swamps, savannahs, and forests that gave them birth, particularly those that are dispersed through the sandstones, or mixed with fishes in the shale beds.

The cases are very few in which I have ever seen fossil trees, or any smaller vegetables erect and petrified in their native place. The Cycadites and stumps of large Coniferous trees on the surface of the oolite in Portland, and the stems of Equisetaceous plants described by Mr. Murchison in the inferior oolite formation near Whitby, and erect plants which I have found in sandy strata of the latter formation near Alencon, are examples of stems and roots overlaid by sediment and subsequently petrified without removal from the spots in which they grew. At Balgray, three miles north of Glasgow, I saw in the year 1824, as there still may be seen, an unequivocal example of the stumps of several stems of large trees standing close together in their native place in a quarry of sandstone of the coal formation.

In a paper on the sinking of the surface over coal mines, Mr. Buddle has shown that the depressions produced on the surface by the excavation of beds of coal near Newcastle-on-Tyne are regulated by the depth and thickness of the coal, the nature of the strata above it, and also the partial or total extraction of the beds of coal. The accumulation of water forming ponds in these superficial depressions, and the sinkings of a railway, have afforded accurate measures of the amount of the subsidences in question.

WEALDEN AND PORTLAND FORMATIONS.

In the north of Germany Mr. Roëmer, of Hildesheim, has identified beneath the Cretaceous system, the Purbeck stone and beds of the Wealden formation, with nearly all its characteristic shells, and three minute species of Cypris. He has also found the Portland sand and the upper and lower Green sand and the Gault clay, in the north of Germany. He has, moreover, found the Wealden formation near Bottingen in the High Alps.

CHALK FORMATION.

In extension of our knowledge of the Chalk formation, the Rev. J. Gunn has sent us a short communication, accompanied by a lithograph representing the columnar disposition of some Paramoudras to the height of many feet one above another in the chalk of Norfolk. The history of these enormous urn-shaped flints, which were first noticed by Professor Buckland in an early volume of our Transactions, 1st series, vol. iv. p. 413. pl. 24., is still involved in much obscurity. Their form is most probably due to siliceous matter collected around and penetrating throughout the substance of gigantic spongiform bodies; but we have yet to learn the reason why they are occasion-
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ally placed in single vertical rows, almost like the joints of a basaltic column, sometimes nearly touching, but not articulating with one another.

A paper has been read by Mr. Henry Lawes Long on the occurrence of numerous *subterraneous chasms* or *swallow-holes* in the chalk on the west of Farnham, with observations on the drainage of the country near the west extremity of the highly-inclined ridge of chalk, called the Hog's Back, between Guildford to Farnham. The land-springs immediately on the north of Farnham descend southwards in open gulleys over tertiary strata, until they arrive at the narrow band of chalk which passes under Farnham Park, where they are suddenly engulfed in transverse fissures or swallow-holes, through which they pass under ground to a considerable distance, and again break forth on the southern side of the chalk. Seven of these swallow-holes occur near Farnham, from some of which the water emerges in sufficient force to turn a mill. They are probably connected with subterranean faults and transverse fractures, the origin of which was coeval with the elevation of the narrow band of chalk, which forms the Hog's Back, and which, near Farnham, is inclined at a high angle to the north. The water that now passes through the Farnham swallow-holes may tend to enlarge the chasms through which it takes its subterraneous course, by dissolving slowly the chalk of their sides in the small quantities of carbonic acid which rain-water usually contains.

Similar transverse fractures, on a greater scale, have given origin to the chasms, which, being enlarged by denudation into transverse valleys, afford outlets through the high escarpment of the chalk to the rivers that, rising within the Weald, flow through the escarpment of the north Downs into the valley of the Thames, and through the escarpment of the south Downs into the sea, viz. to the Wey, the Mole, the Darent, the Medway, and the Stour, through chasms in the north Downs; and to the Arun, the Adur, the Ouse, and the Cuckmere, through chasms in the south Downs.

Dr. Mitchell has communicated a paper on Artesian and other wells, in the gravel and London clay in Essex, showing that water occurs under the London clay at various depths; the deepest at Foulness Island, being 460 feet. He attributes this inequality in part to unevenness in the surface of the subjacent chalk. On reaching the chalk a large volume of water usually rushes up. Artesian wells are now general in Essex, where they are of the greatest utility in districts that have no natural springs. He also gives an interesting list of localities, both of constant and intermitting springs, some of them very powerful, that burst out from the chalk.

Dr. Mitchell has also communicated an account of deleterious gases that occur in wells in the chalk and strata above it near London. The most abundant of these, namely, carbonic acid gas, issues very partially and only from certain strata, and produces sometimes effects fatal to persons employed in digging wells. Sulphuretted hydrogen is occasionally met with in chalk; and both sulphuretted hydrogen and carburetted hydrogen occur in beds immediately above the chalk.

SUPERCRETACEOUS FORMATIONS.

In illustration of the history of the Eocene division of the tertiary strata, Mr. Bowerbank has concluded, from his personal observations at White cliff bay in the Isle of Wight, that there are no well-defined zoological distinctions between the London and plastic clays, but that in the cliffs of this bay the same shells are common to alternations of these clays with one another. At Alum bay also he found many London clay fossils in beds of greenish grey sand and clay below the variegated sands and clays referred by Mr. Webster to the plastic clay. A similar rectification was sometime ago proposed by Professor Sedgwick.

We have also witnessed during the past year the commencement of a valuable publication by Mr. Bowerbank on the fossil fruits and seeds of the London clay, illustrated with very numerous and accurate engravings by Mr. James Sowerby.

The great attention the author has long paid to the remains of fruits and seeds which occur in such vast abundance in the Isle of Sheppy, whence he has collected not less than 25,000 specimens, place him in a position peculiarly advantageous for the object before him. In this work drawings will be given of the anatomical structure of many of these fossils, as seen under the microscope. The simple expedient Mr. Bowerbank has adopted of preserving these fruits in jars of water, has kept him in the entire possession of every specimen ever placed in his collection; whilst of the thousands of similar fossils that have been deposited in other collections, including that at the British Museum, nearly all have perished from the decomposition of the iron pyrites with which they are always penetrated.

Mr. Lyell has communicated to us a paper full of elaborate detail of facts, and of ingenious speculations respecting the Boulder formation, or drift, associated with freshwater deposits, in the mud cliffs of Eastern Norfolk. These cliffs are in some places 400 feet high, and consist of chalk, crag, freshwater deposits, drift mud and sand, stratified and unstratified;—with superficial accumulations of flint gravel. The centre of his observations is the town of Cromer; he considers the Boulder formation to have been accumulated on land permanently submerged, and not, by one or many, transient advances of water over dry land, and therefore proposes, as Mr Murchison and others have already done, to substitute the term of Drift for that of Diluvium, which many other writers have assigned to it. The Drift, or Diluvium, is of two kinds; one composed of sand, loam, clay, and gravel, all regularly stratified; the other consisting of clay, not divided into beds, and containing boulders of granite, trap and other rocks.

This clay is known on the east and north-east coast of Scotland by the name of Till. He considers the stratified Drift and Till to be contemporaneous formations, and compares the latter to moraines formed at the termination of glaciers. He imagines that drifted masses of ice, charged with earthy matter and fragments of rock, may have deposited the Till as they melted in still water, and the

occasional intercalation or juxta-position of stratified materials is ascribed to the action of currents on materials also falling from melting icebergs.

Mr. Lyell refers the complicated bendings and tortuous foldings of many beds of this formation near Mundesley and Cromer to lateral pressure from drifting ice, especially where extremely contorted beds repose upon undisturbed and horizontal strata. But he admits that some of them may be due to landslips of ancient date, and which had no connection with the present line of cliffs. At the bottom of the boulder formation, and immediately above the chalk, extensive remains of a buried forest occur, the stools of the trees being imbedded in black vegetable earth. From the position of this forest a vertical subsidence of several hundred feet and a subsequent rise of the land to the same amount is inferred. This forest and a bed of lignite are connected with fluvial or lacustrine deposits, which occur about the level of low water below the drift; but at Mundesley they are partly above it, and the freshwater shells which they inclose being nearly all of British species, show that they, as well as the contemporaneous drift, all belong to the newer Pliocene period.

In an Address formerly delivered from this chair, in 1836, and in a subsequent edition of his "*Principles of Geology*," as well as in his "*Elements*," Mr. Lyell has called our attention to some differences of opinion which had been expressed by several eminent conchologists as to the number of fossil shells of the crag of Norfolk and Suffolk which could be identified with living species. So great was the discordance of the results at which M. Deshayes, Dr. Beck, and others seemed to have arrived, that their announcement was calculated materially to impair our confidence in the applicability of the chronological test so much relied on by Mr. Lyell for the classification of the tertiary formations; namely, that derived from the proportional number of recent and extinct species discoverable in each deposit. In the hope of arriving at some definite conclusion on this important point, Mr. Lyell visited Norfolk and Suffolk during the last year, and having obtained a considerable collection from the crag near Norwich and Southwold, he instituted, with the assistance of Mr. Searles Wood and Mr. George Sowerby, a thorough comparison between them and recent species. The fossil shells of this formation, which the author calls the Norwich crag, are partly marine, and partly freshwater, and indicate a fluvio-marine origin, and the proportion of living species was found to be between 50 and 60 per cent. This deposit, therefore, the author refers to the older Pliocene period. A similar examination was then made of 230 species of shells from the Red Crag in Mr. Wood's museum, and it was found that 69 agreed with living species, being in the proportion of about 30 per cent. This group therefore Mr. Lyell ascribes to the Miocene era. A collection of 345 species of Coralline Crag shells in Mr. Wood's cabinet was then compared in like manner, and sixty-seven were determined to be identical with recent species, being about 19 per cent. Mr. Lyell, therefore, considers that the Coralline Crag is also

Miocene, although belonging to a more remote part of that period than the Red Crag. Having obtained from M. Dujardin a collection of 240 shells from the Faluns of Touraine, he found with Mr. George Sowerby's assistance that the recent shells were in the proportion of twenty-six per cent., so that he has now come round to the opinion long ago announced by M. Desnoyers, that upon the whole the Crag of Suffolk corresponds in age with the Faluns of Touraine, both being Miocene, although the species in the two countries are almost entirely distinct, those of England having a northern and those of France a sub-tropical character. I am also informed by Mr. Lyell, that out of 400 marine and freshwater species, from the Eocene strata of the London and Hampshire basins, Mr. G. Sowerby was scarcely able to identify two per cent. with living shells. It is satisfactory therefore to observe that the test of age derived from the relative approach to the recent Fauna is in perfect accordance with the independent evidence drawn from superposition. We ascertain for example by superposition that the freshwater strata of the mud cliffs of East Norfolk rest on Norwich crag, and are the newest formation of all. They are then followed in the descending series by, 1st, the Norwich, 2ndly, the Red, and 3rdly, the Coralline Crag, beneath which is the London Clay. The same order of sequence is indicated by the organic remains considered independently, and simply with reference to the degree of their correspondence with the existing Fauna.

It has been known for many years, that near Bridlington, in Yorkshire, sand and clay containing marine tertiary shells had been exposed on the coast. From an examination of the shells collected there by Mr. Bean, Mr. Lyell finds the deposit to agree in age with the Norwich Crag.

I cannot conclude these remarks without observing, that some part of the confusion and apparent inconsistency of the opinions of different conchologists, respecting the age of the Crag, must have arisen from the intermixture of fossils derived equally from the Norfolk and Suffolk beds, or from strata, some of which now turn out to be referable to the Older Pliocene, others to the Miocene period.

From an examination of some fossil shells, identical with recent species collected by Capt. Bayfield from the most modern deposit near the Gulf of St. Lawrence, and near Quebec, Mr. Lyell infers, that the climate of Canada was colder than now during the era immediately antecedent to our own times. The shells, which were determined by Dr. Beck, differ in great part from those now living in the Gulf of St. Lawrence, agree more nearly with arctic genera and species, and resemble those which Mr. Lyell collected at Uddevalla, in Sweden; whereas, if the living shells most abundant in the Swedish and Canadian seas are contrasted, they differ almost entirely. From notes sent by Capt. Bayfield, it appears that at different depths in the stratified sand and clay containing the fossil shells, near Quebec, insulated boulders are numerous, which, it is presumed, have been brought down at distant intervals by

drift ice, and have dropped to the bottom of the sea as the ice melted.

While Mr. Lyell, by the aid of Dr. Beck's determination of fossils, had adopted these views respecting the climate of Canada, Mr. James Smith, of Jordan Hill, had been led by independent observations to a similar conclusion respecting the climate of Scotland during the Newer Pliocene era, arguing from the arctic character of the Testacea found in the raised beds of the valley of the Clyde, and other localities. In the first of two papers communicated by this author, he regarded all the deposits abounding in recent shells in Scotland and Ireland as belonging to one group; but in his second memoir he contends that there are two distinct formations on the Clyde, in the older of which there are from ten to fifteen per cent. of extinct or unknown species of shells, which he refers to the Newer Pliocene system of Lyell; whereas all the species found in the newer, which he calls Post-tertiary, exist also in the present seas. During this Post-tertiary period, which is considered to have been anterior to the human epoch, an elevation of at least forty feet took place on the shores of the Clyde. Mr. Smith affirms that the Till, or unstratified accumulation of clay and boulders, belongs not to the Post-tertiary, but to the older Pliocene division.

IGNEOUS ROCKS.

The principal communication we have received on rocks of igneous origin has been from our Secretary, Mr. W. I. Hamilton, who has read an interesting paper on the north-west part of Asia Minor, from the Peninsula of Cyzicus to Koola, with a description of the Katakekaumene. Between Cyzicus and Koola the principal stratified rocks are schist, with saccharine marble, compact limestone resembling the scaglia of Italy and Greece, tertiary sandstones, and tertiary limestones. The igneous rocks are granite, peperite, trachyte and basalt. The tertiary limestones are referred to the great lacustrine formation which occupies so large a part of Asia Minor. Hot springs burst forth near Singerli from a porphyritic trap rock. The Katakekaumene is a volcanic region, extending about seven miles from north to south, and from eighteen to nineteen east and west. It presents two systems of volcanic craters and coulées: the older of them are placed on parallel ridges of gneiss and mica slate, and the newer in the intervening valleys; hence he argues, that when the latter eruptions took place, the lines of least resistance to subterraneous expansion were in the valleys. The streams of lava from the more recent cones are bare and rugged, like the coulées in central France. Three periods of eruption are traced: the first, having produced basalt, which caps the plains of white limestone, and was ejected before the formation of the valleys; the second, marked by currents of lava from the more ancient system of volcanos in action since the formation of the valleys; the third resembling the coulées of Etna and Vesuvius, and mentioned by Strabo, but of which there is no historical tradition as to the period when they were in activity.

We have a notice by the Rev. W. B. Clarke of a shower of ashes that fell on board the Roxburgh off the Cape de Verd islands in February, 1839, the cause of which was not apparent. The sails were covered with a fine powder, resembling the ashes of Vesuvius, which was probably derived from an eruption in the Cape de Verd group.

PALÆONTOLOGY.

In the department of *Palæontology* Prof. Owen has, during the past year, contributed many papers, with his usual zeal and ability, to the elucidation of this most essential and perhaps most generally interesting branch of our subject. At the head of these we must place his determination of a tooth and part of a jaw of a fossil monkey, of the genus *macacus*, with part of the jaw of an opossum, and the tooth of a bat, in Eocene strata of the English tertiary formation. These remains were found at Kingston, near Woodbridge in Suffolk, by Mr. Colchester, in strata which Mr. Lyell has referred to the London clay; thus proving the existence of quadrumanous, marsupial, and cheiropterous animals in this country during the Eocene period. We have now evidence of fossil *Quadrumana* in the tertiary formations, not only of India and Brazil, but also of France and England; respecting which Mr. Owen has observed, that they appear under four of the existing modifications of the quadrumanous type: viz. the tailless ape (*Hyllobates*), found fossil in the South of France; the gentle vegetable-feeding *Semnopithecus*, found fossil in India; the more petulant and omnivorous *Macacus*, found in Norfolk; and the platyrrhine *Callithrix*, found in Brazil. This genus is peculiar to America, and its extinct species is of more than double the stature of any that exists at the present day. This geographical distribution of *Quadrumana* adds further weight to the arguments derived from the tropical aspect of vegetable remains that abound in the London clay at Sheppy, showing that great heat prevailed in the European part of the world, as well as in India and South America, during the Eocene period.

The probability of high temperature is further corroborated by Mr. Owen's recent recognition of four petrified portions of a large serpent (*Palæophis Toliapicus*), eleven feet long, and in several points resembling a boa, or python; and also of a bird allied to the vultures (*Lithornis vulturinus*), all from the London clay of the Isle of Sheppy; wherein the occurrence of fossil Crocodilians and Testudinata, and of fossil fruits, having a tropical aspect, allied to cocoa-nuts and many other fruits of palms, has been long known. Can we account for these curious facts without supposing that at the Eocene period of the tertiary epoch, the very clay on which London now stands was in the condition of a nascent spice-island, its shores covered with basking reptiles, and the adjacent lands waving with cardomums and palms, and thuias and cypresses, with monkeys vaulting and gamboling upon their branches, and gigantic serpents entwined around their trunks; the seas also swarming with sting-rays and saw-fishes, with chimæras and enormous sharks? for

all these together with countless shells of pearly nautili occur among the fossil remains of the numerous extinct species of fishes, which, during the early ages of the tertiary period, crowded the tepid seas of our now humid and chilling climate.

Mr. Owen has also determined the character of a new genus of Pachydermatous animal (*Hyracotherium*) intermediate between the Hyrax, hog, and Chæropotamus, found in the London clay at Herne Bay, near Margate, by Mr. Richardson.

Mr. Lyell having submitted to Mr. Owen some fossil teeth from the Red Crag of Newbourn in Suffolk, they proved to be referrible to the leopard, bear, hog, and a large kind of deer, and afford the first example of mammalian remains being found in England in any of those divisions of the Crag which Mr. Lyell, in a paper already alluded to, has ascribed to the Miocene period; these genera are known to occur in the Miocene formations of France and Germany. The numerous Mammalia in the fluvio-marine crag of Norwich, are decidedly of a later date; among these Mr. Lyell enumerates the teeth and jaw of *Mastodon longirostris*, a tusk of an elephant with serpulæ attached, and bones of a horse, hog, and field-mouse; there occur bones of birds, many fishes, and numerous shells, partly marine, and partly freshwater and terrestrial.

The recent discoveries in Brazil by Dr. Lund of extinct Mammalia, that probably lived in some late portion of the tertiary epochs, form a new and important chapter in Palæontology. The largest of these are referrible to more gigantic forms than at present exist of families now peculiar to South America—*e. g.* to Sloths and Armadillos; just as most of the fossil mammalia of New Holland belong to families and genera which are still peculiar to that country.* In a paper on one of these animals from Buenos Ayres, Mr. Owen has shown that the bony armour, which several authors have referred to the Megatherium, belongs to the Glyptodon, an animal allied to the Armadillo, and of which a head containing teeth, and attached to a tessellated bony covering of the body and tail, resembling those of an Armadillo, has been lately found near Buenos Ayres, and is figured by Sir Woodbine Parish in his interesting work on that country, 1838.

The Glyptodon differed from the Megatherium in the structure and number of the teeth, and from all known Armadillos in the form of the lower jaw, and the presence of a long process descending from the zygoma; and approached in both these respects to the Megatherium. The teeth differ from those of Armadillos, in having two deep grooves both on the outer and inner surface, are more complex than those of any known Edentate, and indicate a passage from that family into the Toxodon. The ungual phalanges are wholly unlike those of the Megatherium, and most nearly resemble those of *Dasypus*, but are short broad and flat, and seem to have been covered with hoof-like claws. The form of the foot most nearly resembled that of the fore foot of the Mole. Having ap-

[* On this subject see L. & E. Phil. Mag. vol. x. p. 405; vol. xi. p. 208; vol. xii. p. 516.—EDIT.]

propriated to the Glyptodon the armour supposed to belong to the Megatherium, Mr. Owen next proves that the latter animal was unprovided with any such bony covering, arguing from a comparison of its vertebral column and pelyvis with that of the Armadillo; and from the absence of the oblique processes, which in the loricated Edentata resemble as to form and use the *tie-bearers* in carpentry, that support the weight of a roof. The vertebral conditions of the Megatherium are nearer to those of the Sloths and Ant-eaters. We have accounts of twelve skeletons of Megatherium, not one of which was found to be accompanied by bony armour. Cuvier considered the Megatherium more nearly allied to the Ant-eaters and Sloths than to the Armadillos.

Captain Martin has found that many parts of the bottom of the English Channel and German Ocean contain in deep water the bones and tusks of Elephants. They have been dredged up between Boulogne and Dungeness, in the mid-sea between Dover and Calais, and at the back of the Goodwin Sands; also mid way between Yarmouth and the coast of Holland. In 1837 a fisherman enclosed in his net a vast mass of bones between the two shoals called Varn and Ridge, that form a line of submarine chalk-hills between Dover and Calais. Captain Martin says these bones do not occur on the top of banks or shoals, but in deep hollows or marine valleys. Sir John Trevelyan possesses the molars of a large Elephant from gravel in the bed of the Severn, near Watchet, and we have long known that the bones of Elephants occur in great abundance in the oyster grounds off Yarmouth.

In subterranean Ornithology three important discoveries have been made during the past year; the first in the Eocene formation by Professor Owen, who has recognised the fossil Vulture before alluded to in the London clay of Sheppy; the second, by Lord Cole and Sir P. Egerton, who have acquired from the chalk of Kent the humerus of a bird most like that of an Albatross, but of larger and longer dimensions; the third by Professor Agassiz, who has found in Switzerland a nearly entire skeleton of a small bird (not unlike a Swallow), at Glaris, in the indurated blue slate beds of the lower region of the chalk formation. We know that the bones of a Wader, larger than a Heron, have been found by Mr. Mantell in the Wealden formation of Tilgate Forest; and that the Ornithichnites in the New Red Sandstone of Connecticut have been referred to seven species of birds.

We have an interesting accession to our knowledge of the anatomy of the Ichthyosaurus in Mr. Owen's description of the hinder fin of an *Ichthyosaurus communis*, discovered at Barrow-on-Soar by Sir Philip Egerton; this fin distinctly exhibits on its posterior margin the remains of cartilaginous rays that bifurcate as they approach the edge of the fin, showing in this respect a new approximation to the fin of a fish, and more fully justifying the propriety of the name Ichthyosaurus. Traces are also preserved of scutiform compartments on the integument of the fin. It is singular that this structure should never have been observed in any of the numerous spe-

cimens from Dorset and Somerset that have come under our notice ; whilst at Barrow-on-Soar, from whence the paddle in question was derived, even the fibres of the skin and folds of the epidermis are sometimes accurately retained*.

Mr. Owen's first part of his report on fossil Saurians, read at the British Association at Birmingham in August last, forms the commencement of a most important addition to the history of extinct reptiles. His recent investigations in Odontography have also supplied to the geologist a new and most efficient instrument of investigation, enabling him to distinguish genera of extinct animals by the microscopic structure of their teeth ; and as, of all fossil remains, the teeth are the parts most perfectly preserved, and in the case of cartilaginous fishes the teeth and spines are generally the only parts that have escaped decomposition, this method assumes an especial importance in fossil Ichthyology, as affording exact characteristics of animals long swept from the surface of the earth, and whose very bones have been obliterated from among the fossil witnesses of the early conditions of life upon our planet. By this microscopic test applied to the family of Sharks, Mr. Owen has confirmed the views of Agassiz respecting the affinities between the living Cestracion and the extinct genera *Acrodus*, *Ptychodus*, *Psammodus*, *Hybodus*, *Cochliodus* ; in the case of animals also of the higher orders, he has settled the much-disputed places of several extinct gigantic Mammalia by the same unerring test. Thus he has shown the supposed reptile *Basilosaurus* to be a Cetaceous mammifer, allied to the *Dugong* ; the *Megatherium* to be, as Cuvier had considered it, more nearly allied to the Sloth than to the Armadillo ; and the *Saurocephalus* to be, as Agassiz had supposed it, an osseous fish.

Dr. Malcolmson, in a memoir on the Old Red Sandstone of the north of Scotland, has done important service in showing that the rocks composing that group are divided into three formations, the two lower of which are clearly distinguished from each other by their fossil fishes. The cornstone or central formation is charged with numerous remains of Ichthyolites, including *Holoptychus nobilissimus*, a new species of *Cephalaspis*, and other forms not yet described. The lower division, consisting in this region of conglomerates, shales and sandstone, is characterized by the genera *Dipterus*, *Diplopterus*, *Cheiracanthus*, &c., of Agassiz, as well as by the occurrence of a singular Ichthyolite, which seems to offer close analogies to certain forms of Crustacea. By help of these Ichthyolites, the author has been enabled to connect certain strata of Orkney and Caithness, and determine their relations to the beds of Old Red Sandstone containing fossil fishes in the basin of the Tay, and in the border counties of England and Wales, where they had been described by Mr. Murchison.

Mr. Williamson, in a notice on the fossil fishes of the coal-fields of York and Lancaster, says that these coal measures are very rich in Ichthyolites, which abound so much at Middleton colliery, near Leeds, that the workmen have given to one bed the name of fish

* See Buckland's *Bridgewater Treatise*, Pl. 10.

coal ; they are usually in fine bituminous shale above and below the coal, and most frequent in the roof immediately above it, where, as at Burdie House, near Edinburgh, there is a thin seam of coprolitic matter ; they are rarely mixed with any great quantity of vegetable remains. In the lower measures of Lancashire they are associated with Goniatites and Pectens, and in the higher measures of Lancashire and Yorkshire with freshwater shells allied to *Unio*, and with Entomostraca. Exact observations as to facts of this kind are of inestimable importance, for it is only by careful induction from a sufficient number of such-like phænomena, and from similar details as to the local distribution and condition of animal and vegetable remains in the marine and fluvio-marine and lacustrine deposits which compose the carboniferous series, that we shall arrive at a solution of the grand problem of the formation of coal.

CRUSTACEANS.

The Rev. T. B. Brodie has discovered in the Wealden formation near Dinton, in the vale of Wardour, the remains of Coleopterous and Hymenopterous insects, and a new genus of *Isopodous Crustacea* in the family Cymothoidæ. The Isopods are clustered densely together ; the lenses in their eyes are sometimes preserved ; there are also traces of legs, but of no antennæ. With them he has found a large species of *Cypris*. The insects are chiefly small Coleoptera ; there are several species of Dipterous, and one Homopterous insect, and the wing of a *Libellula*. Mr. Brodie's discovery is the first yet made of insects in the Wealden formation, and also the first example, in a secondary formation, of Isopods that approximate in form to the Trilobites of the Transition series.

WORMS.

An addition has been made to fossil Helminthology by Mr. Atkinson of Newcastle-on-Tyne, who has found in slabs of micaceous slaty sandstone, from the carbonaceous series near Haltwhistle, tortuous casts of vermiform bodies of various sizes, some almost an inch in diameter, and several feet in length ; the surface of many of these is thickly marked by transverse rings and a longitudinal groove, similar to those in the largest recent marine sand worms, *e.g.* the *Leodice gigantea*. The integument of some of these worms containing *chitine*, like the covering of insects, seems to have endured long enough to fix impressions of the transverse rings upon the sand ; and the habit of swallowing large quantities of earth and sand, which we observe in many recent worms, may explain the presence of the large portion of sand, now indurated to stone, which occupies the interior of the impression of the skin. Since many casts are found upon the same slab, these worms must have been very numerous at the bottom of the sea, when the sandstone was in process of formation. Similar impressions of Annelids on the Cambrian rocks are figured by Mr. Murchison in Pl. 27 of his great work on the Silurian System.

ICHOLOGY.

About twelve years ago we witnessed the creation of a new department in geological investigations, viz. the science of Ichnology, founded on the evidence of footsteps made by the feet of animals upon the ancient strata of the earth; this new method commenced with the recognition of the footmarks of reptiles on the New Red Sandstone near Dumfries, and not long after (1834) was followed by most curious and unexpected discoveries in Saxony and America. The *Chirotherium* of Hessberg and *Ornithichnites* of Connecticut were among its early results. Our own country has during the last two years been abundantly productive of similar appearances in many localities.

In recent excavations for making a dock at Pembray, near Llanelly, in Pembrokeshire, tracks of deer and of large oxen have been found on clay subjacent to a bed of peat, the lower peat being moulded into the footsteps; similar impressions were also found upon the upper surface of the peat beneath a bed of silt, and bones both of deer and oxen in the peat itself. Footmarks of deer have been also noticed in Mr. Talbot's excavations for a harbour near Margam burrows on the east of Neath.

Near Liverpool Mr. Cunningham has successfully continued his researches begun in 1838, respecting the footsteps of *Chirotherium* and other animals in the New Red Sandstone at Storeton Hill, on the west side of the Mersey. These footsteps occur on five consecutive beds of clay in the same quarry, the clay beds are very thin, and having received the impressions of the feet, afforded a series of moulds in which casts were taken by the succeeding deposits of sand, now converted into sandstone. The casts of the feet are salient in high relief on the lower surfaces of the beds of sandstone, giving exact models of the feet and toes and claws of these mysterious animals, of which scarcely a single bone or tooth has yet been found, although we are assured by the evidence before us of the certainty of their existence at the time when the New Red Sandstone was in process of deposition.

Further discoveries of the footsteps of *Chirotherium* and five or six smaller reptiles in the New Red Sandstone of Cheshire, Warwickshire and Salop, have been brought before us by Sir P. Eger-ton, Mr. I. Taylor, jun., Mr. Strickland, and Dr. Ward.

Mr. Cunningham, in a sequel to his paper on the footmarks at Storeton, has described impressions on the same slabs with them, derived from drops of rain that fell upon thin laminae of clay interposed between the beds of sand. The clay impressed with these prints of rain drops acted as a mould, which transferred the form of every drop to the lower surface of the next bed of sand deposited upon it, so that entire surfaces of several strata in the same quarry are respectively covered with moulds and casts of drops of rain that fell whilst these strata were in process of formation.

On the surface of one stratum at Storeton, impressed with large footmarks of a *Chirotherium*, the depth of the holes formed by the

rain drops on different parts of the same footstep has varied with the unequal amount of pressure on the clay and sand, by the salient cushions and retiring hollows of the creature's foot; and from the constancy of this phenomenon upon an entire series of footmarks in a long continuous track, we know that this rain fell after the animal had passed. The equable size of the casts of large drops that cover the entire surface of the slab, except in the parts impressed by the cushions of the feet, record the falling of a shower of heavy drops on the day in which this huge animal had marched along the ancient strand; hemispherical impressions of small drops, upon another stratum, show it to have been exposed to only a sprinkling of gentle rain that fell at a moment of calm.

In one small slab of New Red Sandstone found by Dr. Ward near Shrewsbury, we have a combination of proofs as to meteoric, hydrostatic, and locomotive phenomena, which occurred at a time incalculably remote, in the atmosphere, the water, and the movements of animals, and from which we infer with the certainty of cumulative circumstantial evidence, the direction of the wind, the depth and course of the water, and the quarter towards which the animals were passing; the latter is indicated by the direction of the footsteps which form their tracks; the size and curvatures of the ripple-marks on the sand, now converted to sandstone, show the depth and direction of the current; the oblique impressions of the rain drops register the point from which the wind was blowing, at or about the time when the animals were passing.

Demonstrations founded solely upon this kind of circumstantial evidence were duly appreciated, and are well exemplified, by the acute author of the story of Zadig; who from marks he had noticed on the sand, of its long ears, and teats, and tail, and from irregular impressions of the feet, declared the size and sex, recent parturition and lameness of a bitch he had never seen; and who from the sweeping of the sand, and marks of horse-shoe nails, and a streak of silver on a pebble that lay at the bottom of a single footstep, and of gold upon a rock against which the animal had struck its bridle, inferred that a horse, of whose existence he had no other evidence, had recently passed along the shore, having a long switch tail, and shod with silver, with one nail wanting upon one shoe, and having a bridle studded with gold of twenty carats value.

In addition to the commencement of Mr. Bowerbank's publication on the Fossil Fruits and Seeds of the London Clay, before alluded to, we have hailed with satisfaction the announcement, by Professor Henslow and Mr. Hutton, of their intended continuation of the Fossil Flora of Great Britain, conducted for some years by Dr. Lindley and Mr. Hutton, and lately suspended.

A Dictionary of the terms and language of geology has long been a desideratum to young students, to whose early progress the technical terms of the science have hitherto presented formidable impediments. This want has been recently supplied by two publications of this kind, one by Mr. George Roberts, author of the History of Lyme Regis; the other by Dr. Humble.

During the last year the Society has received no communication on Mineralogy; and almost the only volume that has been published in England on this much-neglected subject, has been a small but highly elaborate treatise on Crystallography by Professor Miller, of the University of Cambridge. In this treatise the author has adopted the crystallographic notation proposed by Professor Whewell in his paper on a General Method of calculating the Angles of Crystals, and the laws according to which they are formed, published in the Transactions of the Royal Society of London, 1825; and Professor Naumann's method of indicating the positions of the faces of a crystal by the points in which radii, drawn perpendicular to the faces, meet the surface of a sphere. The expressions which have been thus obtained are remarkable for their symmetry and simplicity, and are all adapted to logarithmic computation, and for the most part new.

NOTICE OF DECEASED MEMBERS.

In proceeding to speak of the losses which, during the past year our science has sustained by death, I shall offer my first tribute of respect to the memory of one, whom a predecessor of mine in this chair has justly called the father of English geology; since to his discoveries we owe the first diffusion of exact knowledge as to the order of superposition of the secondary formations which occupy so large a portion of our island, and the first demonstration of that constancy of the organic remains, which he proved to be characteristic of the component strata of each different formation. It was the especial merit of Mr. WILLIAM SMITH to establish a series of types of these groups, many of which have been adopted as classical, in such a manner as will perpetuate his name among the original discoverers of the age in which he lived.

If, as it has been truly said, the honour of the first discoveries in tertiary geology belongs to France, where the labours of Cuvier and Brongniart gave to this great division of the strata of the earth a systematic arrangement before unknown, so the establishment of the types in secondary geology, from the chalk down to the new red sandstone, is due to England; and the discovery of the leading natural divisions of that important portion of them which constitutes the oolite formations, was almost exclusively the work of Mr. William Smith.

His earliest publication was a treatise on irrigation, 1806, a subject on which his experiments gained him a medal from the Society of Arts.

In 1801 he printed proposals for publishing accurate delineations and descriptions of the natural order of the various strata that are found in different parts of England and Wales, to be illustrated by a small geological map*. This work was never completed, but it led to the publication of his large map, in 1815, for which the Society

* The original coloured copy of this map, dated 1801, was presented by Mr. Smith to our Society, and is now in the Museum.

of Arts awarded him their medal and a premium of £50. In the same year also his stratigraphical collection of organic remains was purchased for the British Museum; this collection having formed the basis of his two separate volumes, entitled "Strata identified by their Organized Fossils," 1815, and "a Stratigraphical System of Organized Fossils," 4to, 1817.

During the six years which followed the publication of his map of England, he put forth twenty geological maps of English counties on a larger scale, and several coloured sections across the south of England, and a general Geological Section of England and Wales, from London to Snowdon.

Among his unpublished papers were found unfinished and in part printed, an introductory work on geology, and preparations for a volume on Economic Geology, both illustrating the originality of his views.

MR. WILLIAM SMITH entered on the field of his honourable exertions as a Civil Engineer and Mineral Surveyor at a time when his labours in geology were but little appreciated, and almost solitary. Amidst difficulties and discouragements, and at intervals snatched from the duties of a laborious profession, he accomplished the gigantic work of a general mineralogical survey of England, founded almost entirely on his own personal observations, which he ultimately recorded in a map of fifteen coloured sheets, published by subscription in 1815.

Inevitable delays retarded the appearance of this work nearly to the time when a more detailed and perfect map, by a distinguished president of this Society, eclipsed in some degree the fame which would have accrued to its author had it been published earlier, even in the less perfect form to which he had advanced it some years before. The sense entertained by this Society of the value of the scientific services of Mr. Smith, was marked by their award to him of their first Wollaston Medal, in 1831; and was accompanied by the just and eloquent eulogium pronounced on that occasion by Professor Sedgwick. In the same year also the British Association assembled at York made successful application to government for a pension, which was settled upon Mr. Smith for life; and at the meeting of this Association at Dublin, 1835, the University conferred on him the honorary degree of Doctor of Civil Law.

Mr. Smith was one of those remarkable persons whom strong natural sense and acute powers of observation occasionally enable to triumph over the disadvantages of a defective education. His attention was first called to physical inquiries, by the observing, when a boy, that a large stone which he was lifting under water in search of eels, could be moved with much more ease, than if the same stone had been on land. His juvenile curiosity was excited to learn the cause of an occurrence so surprising to him; and this first step led him, at the age of eighteen, to enter the profession of a surveyor and civil engineer. His early professional occupations from the year 1791 to 1799, whilst surveying collieries, constructing a part of the Somerset coal canal near Bath, and preparing reports

respecting a supply of water for the Kennet and Avon Canal, and the trade it was likely to derive from carriage of stone and coal, &c., placed him in daily contact with geological phenomena especially calculated to illustrate the order of superposition of the English strata, and laid the foundation of his future discoveries.

By carefully noting the characters of the beds which he found in juxtaposition, and making comparative sections in various directions in the vicinity of Bath, he ascertained that an uniform order of succession pervades the groups exposed in the escarpments of the hills in that part of England, and that this uniformity is attended by a similarity in the organic remains of certain beds, which differ entirely from those of the groups above and below them; by diligently collecting and collating these remains, he drew the inference, that each group of strata contains extraneous fossils peculiar to itself.

His next step was to infer that the strata thus identified by himself in Somerset and Wiltshire were not of insulated and local occurrence, but formed parts of the great system of deposits extending over England; and thus, after many years of intense labour and continual travel, he succeeded in extending the principles first caught sight of in the neighbourhood of Bath, into that philosophical generalization which became the basis of his geological map of England.

— Before Mr. Smith had quitted his occupations in Somerset and his residence at Bath, he indicated on a coloured map the geological structure of that neighbourhood. This document, dated 1799, is in the museum of our Society. He had also arranged his collections of rocks and their organic remains in the order of succession and continuity of the several strata; but neglecting to appropriate to himself the merit of these discoveries by immediate publication, he liberally imparted a knowledge of each, as it gradually arose, to his private friends, through whose oral communications they obtained such general currency, that their real author was frequently lost sight of or unknown. I was myself indebted to Mr. Smith, though at that time a stranger to me, for my first knowledge of the order of succession in the oolitic series. This I derived from information imparted to me by the late Rev. B. Richardson of Farley Castle, who had himself acquired it from Mr. Smith. A tabular view of the superposition of the English strata, written by Mr. Richardson, from the dictation of Smith in 1799, at the house of the Rev. Joseph Townsend, in Bath, and since also presented to this Society, forms a documentary proof of the extent of his discoveries before the conclusion of the last century.

In 1817 he planned the beautiful museum of Scarborough, in which he employed his original and instructive method of representing, by sloping shelves passing one beneath another, the inclined position of the strata; each shelf bearing the fossils that are respectively characteristic of the stratum it is intended to represent.

These works of William Smith undoubtedly place him in the position of an original discoverer, who was the first to establish, on an enlarged basis of evidence, the important facts of constancy

in the order of superposition, and continuity in the horizontal extension of the strata of this island; and to prove that each of these strata is characterized by organic remains peculiar to itself. But it must not be forgotten, that both in this country and on the continent, other investigators, many of them no doubt unknown to him, were simultaneously collecting similar evidence in support of this great physical generalization. It only enhances the value and confirms the accuracy of Mr. Smith's conclusions, that the results of other independent inquiries were found to be in perfect harmony with his own. It is known to all who are acquainted with the productions of the school of Freyberg, that Werner had pointed out the importance of petrifications as affording a basis for the arrangement of geological formations, the same in principle, though confirmed by less extensive details, than those which Mr. Smith elicited from the oolitic series in England. Professor Jameson has expressly stated that Werner was aware that petrifications are comparatively rare in the transition rocks, increasing in number in the newer series of that division, and becoming still more numerous in the Floetz formations: he had further remarked, that the animals of the earliest periods are of the lowest and most imperfect class, namely zoophytes; that in ascending through newer and newer formations, we meet with shells and fishes and marine plants, all different from any living animals and vegetables of the present earth; that in the newest formations we find the remains of existing genera with those of land animals and land plants.

Werner had also noted, in some detail, the order of succession of the strata of the Muschel-kalk of Germany, founding his divisions upon the changes he observed in the petrifications it contains; and thus announcing the principle of making distinctions in strata upon the nature of their organic remains.

The same principle had been previously caught sight of and partially elaborated by Lehman in Germany, and by other observers in France, where its application to tertiary strata received the fullest demonstration, in the great discoveries of Cuvier and Brongniart within the basin of Paris. In our just admiration of our countryman, therefore, we must not lose sight of the merits of his contemporary labourers on the continent; and whilst we honour him as the father of English Geology, let us also pay just homage to those who had started before him in the same course, wherein it was his undisputed merit to have arrived first at the goal.

Mr. W. Smith was born on the oolite formation at Churchill, in the county of Oxford, in 1769. When a child he was in the habit of collecting *Terebratulæ* from the oolite rocks in the fields of his native village, which he used as substitutes for marbles.

As an engineer he was employed in works of irrigation and drainage in many parts of England; as well as in stopping out the sea from breaches through which it had invaded the marshes of Norfolk, 1806, 1807, &c., and in the draining off the water of Mismer lake in Suffolk into the sea. He was the engineer also of the Ouse navigation in Sussex. In 1809 he was engaged in the restoration of

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the hot springs at Bath. In 1821 he recommended to Col. Braddyl to search for coal (beneath the magnesian limestone) on an estate in which is now situated the great South Hetton Colliery. No colliery in Northumberland had been worked, at that time, under the magnesian limestone.

Mr. Smith's principles of drainage have been applied with much advantage near Bath, Woburn, and in Norfolk. Finding the town of Scarborough to be very ill supplied with water, he excavated in the interior of the hill of Falsgrave Moor, two or three miles distant, a subterranean reservoir, in which he collected, from streamlets percolating that hill, sufficient water for the permanent supply of the town*.

From his early days to the latest period of his life he tells us that he had the habit of looking on the ground†.

Mr. Smith's last public employment was in conjunction with Mr. De la Beche and Mr. Barry, in the Commission for reporting on the best building-stone for the new House of Commons‡. During the later years of his life he resided near Scarborough superintending the estates of Sir John Johnson at Hackness; and dying at Northampton, in August 1839, aged seventy-one, after a few days' illness, at the house of his friend Mr. Baker, the historian of Northamptonshire, on his way to the Meeting of the British Association at Birmingham, was interred in the church-yard near the west end of the beautiful Norman church of St. Peter, in Northampton, which stands on the Oolite formation. He had often expressed a wish to be buried in this formation, on which he was born and educated, and the history of which he had so much elucidated. A monument will be erected to his memory in St. Peter's Church by subscription of members of the Geological Society of London.

It was not the least of the services which have been rendered to our science by Mr. Smith, that he was during many years the geological preceptor of his accomplished nephew Mr. John Phillips, in whom he has bequeathed to us a pupil, who has shown, by publications of the highest order in various departments of Geology, the soundness of the instructions received from his affectionate uncle.

Mr. DAVIES GILBERT was one of the earliest members elected into this Society, at its formation in 1808. During two years he served as a Vice-President, and for six years was a member of our

* An account of this curious work is published by himself in the *Philosophical Magazine* for June 1827.

† See a paper by himself on Quartz in Soils, published in *Charlesworth's Magazine* for July 1837.

‡ For more detailed accounts of the life of Mr. Smith, and of the amount and value of the services he rendered to Geology in England, I must refer to Dr. Fitton's masterly and candid investigation of this question in the *Edinburgh Review*, Vol. XXIX, p. 310, &c. [reprinted, with additions, in *L. & E. Phil. Mag.*, vol. i. p. 147, &c.]; to Mr. Conybeare's Introduction to his *Outlines of the Geology of England and Wales*, 1822, p. 45; to the Address of Professor Sedgwick to this Society, 1831; and to a biographical notice by his nephew Professor John Phillips, in *Charlesworth's Magazine of Natural History*, New Series, 1839, p. 213.

Council; and though he communicated no papers, he took a lively interest in all our proceedings, and was ever prompt on all public occasions to promote the welfare and forward the great objects of our institution.

His paternal name was Giddy: he was descended in the line of both his parents from very respectable families in Cornwall, and on the maternal side of Davies, allied to the noble family of Sandys; in 1817 he assumed the name of Gilbert, on succeeding to the property of his wife's uncle, Mr. Charles Gilbert, of East Bourne, in Sussex.

Having been privately educated in Cornwall, he became, in 1785, at the age of eighteen, a gentleman-commoner of Pembroke College, Oxford, where, being of more studious habits and more mature attainments than is usual with students of his age, he associated chiefly with the senior members of his College. Dr. Parr, writing at this time to the late master of Pembroke, speaks of Mr. Giddy, then twenty-three years old, as "the Cornish philosopher," and adds, that "he deserves that name."

To this College, as well as to the University, his affectionate and devoted attachment endured to his latest hour, and he became on several occasions a liberal benefactor towards improvements in Pembroke and its vicinity. During many years it was his great delight to pass a few days at Oxford, and he always considered the diploma Degree of Doctor of Laws, conferred on him by the University in 1832, as one of the most gratifying events of his life.

During his early residence his taste for chemistry and other branches of physical science had introduced him to the acquaintance of Dr. Beddoes, at that time a resident Member of Pembroke College, and who subsequently dedicated to him his pamphlet on mathematical evidence. This acquaintance* was the remote cause of the first step in the public life of Sir Humphry Davy; when Mr. Giddy, who had discovered young Davy's genius for chemistry whilst yet a boy at Penzance, introduced him to Dr. Beddoes, to assist in his laboratory at Bristol, little dreaming that he should himself one day become the successor of this boy in the chair of the Royal Society.

Mr. Davies Giddy was elected a fellow of the Royal Society in 1791, and subsequently of the Antiquarian, Linnean, Geological, and Astronomical Societies of London. He was also an honorary member of the Royal Society of Edinburgh, the Royal Irish Academy, and of the New University of Durham. In 1814 he was elected first President of the Royal Geological Society of Cornwall, and afterwards Vice-Patron of the Cornwall Royal Polytechnic Society, in both which offices he continued till the day of his death. He held the distinguished office of President of the Royal Society, during three years, from 1827 to 1830, and contributed several important papers to their Transactions; one upon the Mathematical

* [He also enjoyed the friendship of Dr. Priestley: and he was on his way to Birmingham on a visit to him, when, at a short distance from the town, he learnt that the residence of his venerable friend was in flames, and that a bigoted mob were in the act of destroying the library, manuscripts, and laboratory of that excellent man and distinguished philosopher.—ED.]

Theory of Suspension Bridges (vol. 116, 1826, Part I., p. 202); also a Table for facilitating the Computations relative to Suspension Bridges (vol. 121, 1831, p. 341); a third paper, entitled Observations on Steam Engines (vol. 117, 1827, p. 25); and a fourth on the Efficacy of Steam Engines in Cornwall, with Investigations of the Methods best adapted for imparting great Angular Velocity (vol. 120, 1830, Part I., p. 121); likewise a paper on the nature of Negative and Imaginary Quantities (vol. 121, 1831, p. 91). He also printed three Addresses as President of the Royal Society, 1828, 1829, 1830*.

In 1804 he was returned to parliament for the borough of Helston; and in 1806 for Bodmin, which place he represented till 1832. During that time he was continually called on by the House of Commons to serve on committees of inquiry touching scientific and financial questions, on which latter subject he published a letter, entitled "A plain Statement of the Bullion Question." He was Chairman of the Committee for rebuilding London Bridge, which he caused to be widened ten feet. The rectification of the national standards of linear dimensions and capacities, was undertaken upon his motion for an address to the Crown.

In his native county also, his authority was continually appealed to on scientific questions, and calculations of practical importance in the machinery of mines and steam-engines; and he was ever ready on all occasions to devote his time and talents to the service of his friends and of the public. In 1792, on the occurrence of a riot in Cornwall, whilst he was a young man, holding the office of sheriff, there being no soldiers in the county, he performed, for the last time that such an event has occurred in England, the military duty of calling out the *posse-comitatus*.

Few persons excelled Mr. Gilbert in bringing the results of much contemplative study to bear on the business of life; his strong point lay in the application of high mathematical knowledge to practical purposes, and in calculating the amount of effective power to be derived from the use of mechanical forces, judiciously combined. For the exercise of this talent his beloved native county offered unusual opportunities; it also afforded him abundant materials for gratifying his taste for antiquarian researches; and the fruits of his labours as a biographer and local historian were presented to the public in 1838, in four 8vo vols.; this work is entitled *The Parochial History of Cornwall*, founded on the manuscript histories of Mr. Hals and Mr. Tonkin, with additions and various

* Mr. Gilbert was also the author of the following papers in the Quarterly Journal of Science and the Arts: Observations on the properties of the Catenarian Curve with reference to Bridges by Suspension, vol. x. p. 230; On the Ventilation of Rooms, and the Ascent of Heated Gases through Flues, vol. xiii. p. 113; Investigation of the Methods used for approximating to the Roots of Affected Equations, vol. xiv. p. 353; Researches on the Vibrations of Heavy Bodies in Cycloidal and Circular Arches, vol. xv. p. 90; On the General Nature and Advantages of Wheels and Springs for Carriages, the Draft of Cattle, and the Form of Roads, vol. xviii. p. 95; On the Vibration of Heavy Bodies, vol. xx. p. 69.

appendices by himself, and brings down the account of families and descent of property in that county from the death of those biographers, about the middle of the last century, to the present time. Mr. Gilbert's additions and criticisms form no small part of its value; he has introduced also copious scientific notices by Dr. Boase and other modern authors, relating to the geology of the county, a subject, he observes, of such recent origin, that the very word does not occur in Chambers's *Encyclopædia* printed in 1783. In acknowledgment of his indirect influence upon this science, I am bound to state with gratitude that my *Bridgewater Treatise* would never have existed, had not the appointment to write it been conferred upon me by Mr. Gilbert whilst President of the Royal Society.

Mr. Gilbert was an assiduous collector of ancient traditions, legendary tales, songs and carols, illustrating the manners, sports, and pastimes of the peasantry of Cornwall; and he was a writer of several anonymous letters and papers in the *Gentleman's Magazine*. He possessed great memory and powers of quotation and anecdote, enriched by vast stores of traditional information as to the personal history of many of the most distinguished individuals of his time, much of which will have perished with him. It has been truly said of him by a contemporary biographer, that "His most endearing talent was his power of conversation. It was not brilliant; it was something infinitely beyond and better than mere display; it was a continued stream of learning and philosophy, adapted with exquisite taste to the capacity of his auditory, and enlivened with anecdotes to which the most listless could not but listen and learn.

"His manners were most unaffected, child-like, gentle, and natural. As a friend, he was kind, considerate, forbearing, patient, and generous; and when the grave was closed over him, not one man, woman, or child, who was honoured with his acquaintance, but will feel that he has a friend less in the world. Enemies he can have left not a single one."

During the last twelve months his strength had been rapidly declining, but he retained full possession of his intellectual faculties till within a few hours of his death; he breathed his last in the bosom of his family at East Bourne, on the 24th of December last, in the seventy-third year of his age. An exact and admirable representation of his finely-formed head and intelligent countenance is preserved in a bust by Westmacott in the Hall of Pembroke College, Oxford.

Sir JOHN ST. AUBYN, who died during the last year, was one of the founders and early Vice-Presidents of the Geological Society, and was among its most firm and valuable friends and supporters at that perilous moment of its existence when the struggles and opposition which attended its first establishment had nearly crushed it in the bud; he was also a liberal contributor to the supplies at that time requisite for its advancement.

He subscribed largely also to the funds then raised for the publication of Count Bournon's crystallographic work on Carbonate of Lime, and for enabling Dr. Berger to undertake his tours in Cornwall, preparatory to his geological description of that county.

The meetings held for the purpose of forwarding Count Bournon's work by some of the most distinguished mineralogists of that day, when collections in geology were rare, was one of the steps that brought together our first founders: many of them were till then strangers to each other, and being thus accidentally introduced, they resolved from thenceforth to cooperate for the furtherance of objects in which they felt a common interest, and became the germ of the Geological Society.

Sir John St. Aubyn was at this time occupied, like his friends Sir Abraham Hume and Mr. Greville, in making large and costly additions to his cabinet of simple minerals, the nucleus of which consisted of the specimens he had purchased of Dr. Babington in the year 1799, and which are described by Babington in his catalogue (one vol. 4to) published in the same year. These specimens had previously been the property of Lord Bute.

The position of his seat at Clowance, in the centre of the greatest mining district of Cornwall, afforded facilities for acquiring the most choice productions of that great repository of mineralogical treasures, and of these facilities he assiduously availed himself during many years. His other seat on the picturesque granitic pinnacle of St. Michael's Mount in the bay of Penzance (the Ictis of Diodorus, from whence the Romans exported tin to Gaul), placed him in another position of high geological and mineralogical advantages; the granite veins that intersect the killas at the base of this classic mountain being among the first described and most instructive instances which Cornwall affords, of the important phænomena of the injection of granite into slate, and the metamorphic condition of the slate thence resulting; whilst a well-exposed tin vein at the base of the ancient fortress and monastery that crown this insulated mountain, affords specimens of Apatite, and is more richly studded with minute but perfect crystals of topaz than any other vein known to exist in this country. These easily accessible examples of phænomena, most highly interesting to the mineralogist and geologist, he carefully preserved for the inspection of the numerous visitors that are continually attracted to this spot—of threefold interest, to the antiquary, the artist, and the mineral philosopher.

A similar zeal for the preservation of interesting scientific objects induced Dr. Jenner to preserve, for the benefit of geological visitors, a rock which presented the rare phænomenon of organic remains intermixed with toad-stone, on the side of a trap dyke intersecting old red sandstone at Newport, near his residence at Berkeley.

To the nucleus formed by Dr. Babington's collection, Sir John St. Aubyn made large additions, not only from the productions of Cornwall, but also from foreign countries, particularly the mines of Germany and Hungary, many of which are no longer wrought. This collection was very rich in the ores of gold, silver, copper, and other metals, and particularly in native diamonds and gems. The arrangement of it was begun by Count Bournon, but subsequently completed after the system of Mohs.

In 1834 he presented the bulk of his collection to the Devonport Civil and Military Library, of which he had been annually appointed

President from its formation in 1827 until his death; and a collection of Duplicates to the museum of Saffron Walden, near which place he then resided. He was an active member of the Geological Society of Cornwall, and of many scientific institutions in London; had a knowledge of Chemistry, Conchology, and Botany; and was a patron of the fine arts and a collector during his whole life.

In Brigadier CHARLES SILVERTOP the Society has lost the author of many interesting communications to our Evening Meetings on the Geology of Spain, the mineral structure of which, notwithstanding its proximity to France and England, and the long-continued military operations of both these nations upon its territory, is less known than that of any other portion of civilized Europe.

The unhappy circumstances of the country have long abstracted the attention of the Spaniard from researches of science, and the difficulties of travelling in the midst of civil commotions have deterred even the enterprising spirit of neighbouring geologists from endeavouring to fill up the lamentable blank which Spain still presents upon the scientific map of Europe.

Brigadier Silvertop, though occupied in the professional engagement of arms, was not forgetful of the pursuits of science. He published the substance of his communications to this Society in a small volume, 1836, wherein he gives a sketch of the widely-disseminated deposits of tertiary beds in the provinces of Granada and Murcia, accompanied by a general view of the volcanic and other rocks of the same district, illustrated by sections, which represent the configuration of the ground, the relative height of the ridges, and the superposition of the strata. He died at Rennes, in June last, on his way to the Pyrenees and Italy.

Mr. LOUIS HUNTON was the author of a paper printed in our Transactions on the Upper Lias and Marl-stone of Yorkshire, showing the limited vertical range of the species of Ammonites and other Testacea, and illustrating their value as geological tests. His observations are founded on the details of the section of Easington height, near Whitby.

JENS ESMARK, Professor of Mineralogy in the University of Christiania, was one of the many disciples of the school of Freyberg, who imbibed from their master an enthusiastic devotion to his theories, which largely contributed to stimulate into activity that general spirit of geological inquiry, the expansion of which, during the present century, has produced such unexpected and extensive discoveries in the development of the structure of the earth.

In 1794, deeply imbued with the doctrines of Werner, he went to Vienna to prepare himself for a tour through Hungary; after this he remained some months at Chemnitz, and visited the other chief mining districts of Hungary, Transylvania, and the Bannat, and crossing the Carpathians to Wielitzka and Cracow, returned to Saxony by the mines of Tarnovitz in Silesia.

In 1798 he published, at Freyberg, the result of his observations, in a small octavo volume, giving descriptions of the mines he visited, and their respective productions, and expressing his conviction of

the truth of Werner's opinion as to the Neptunian origin of the pumice and obsidian (even that of the Lipari Islands), as well as of trap and granite. A translation of his remarks on the *Geological History of the Globe* was published in the *Edinburgh New Philosophical Journal* (1827), vol. vi. p. 107. The most important portion of this paper consists in its bearing his evidence to show that the greater part of Norway has, at some period, been covered with ice, and that the granite blocks, so abundant in that country, have been brought to their present place by glaciers.

In 1829 Professor Esmark published a *Tour in Norway**, containing many measurements of heights, and he was the first to measure the lofty mountain of Schneehätten. He also published various detached *Memoirs on Mineralogy*†.

He is said by Otte to have been the first discoverer of chromate of iron in Norway; and the Norwegian datolite, which was also discovered by him in 1806, was at that time named Esmarkite. He published a short notice on tellurium, in the 3rd vol., 1st series, of our *Transactions*.

His residence at Christiania, in the vicinity of iron, copper, and silver mines, and of the School of Mines and Agriculture at Kongsberg, gave full scope to his taste and talents, and also afforded occasion for the exercise of those courteous attentions which have, during many years, been gratefully acknowledged by scientific travellers in Norway.

He once came to England, and was a member of the Wernerian Natural History Society of Edinburgh.

He was an excellent chess-player; and in appearance, countenance, and the fine form of his head, resembled Mr. Davies Gilbert, whom it has been my painful duty to associate with him in the catalogue of the losses we have sustained during the last year.

DON CARLOS DE GIMBERNAT, Member of the Royal Academy of Sciences at Munich, was the son of a physician of Barcelona, and, from political motives left his native country at the commencement of the French Revolution for Paris, where he passed many years. He had previously studied at Freyberg under Werner, and visited England, where he became acquainted with Townsend, our Spanish traveller, and with Dr. Hope, of Edinburgh; giving to the physical sciences the attention usually required of students for the medical profession, and continuing to cultivate them in his later years. He was more particularly attached to Chemistry, Geology and Mineralogy, and analysed the waters of many hot mineral springs, and found azote in all. The medical virtues which he ascribed to these springs raised him high in the estimation of the Swiss.

M. Gimbernat published accounts of his discovery in the thermal waters of Aix in Savoy, Baden, and other warm springs in Switzerland, of a mucous organic substance, (formed, as he fancied, by chemical precipitation, from azote and carbonic acid,) which he thought was more nearly allied to animal than vegetable matter, and

* Reis von Christiania nach Drontheim.

† In the *Magazin for Naturvidenskaberne*.

to which he gave the name of Zoogene; and he also supposed that he found the same substance in the thermal waters of Ischia, and in waters produced by the condensation of the steam disengaged from Vesuvius. A similar mucous substance, in the thermal sulphureous waters of Roussillon, was supposed by Professor Anglada, of Montpellier, to be a chemical product, from elements held in solution by the waters at the time they issued from the earth, and deposited by them in a flocculent form when they come in contact with the air. De Saussure, however, Decandolle, Dillwyn, and Daubeny*, founding their opinions on the structure it exhibits under the microscope, refer this gelatinous substance to minute *Confervæ*; but the more recent discovery, by Ehrenberg, of infusorial animals in the warm springs of Bohemia, gives some probability to the supposition that these may be mixed with *Confervæ* in the so-called zoogene of Gimbernath. The decomposition either of *Confervæ* or of Infusoria would afford the azote found in zoogene; but their presence would transfer the origin of this organic substance from simple chemical agency to the instrumentality of organic life. On quitting Naples, in 1820, he retired to Switzerland, where he fell into bad health and reduced circumstances, and died at Geneva in 1839†.

FREDERICK MOHS, Professor of Mineralogy in Vienna, was born at Gernrode, in the Harz Mountains, about 1770. He lost his father, a merchant, very early, and was expected to succeed him in the business; but his predilections for science, particularly for mathematics, had marked him out for higher destinies.

He began his studies, 1796-98, at Halle, and continued them in the mining institution at Freyberg.

We find him in 1802 at Vienna, occupied in describing the mineral cabinet of the banker Von der Nüll, where he first conceived those views which he afterwards developed in his system of mineralogy.

His fondness for geology and the art of mining induced him to visit Styria, Salzburg, Carinthia, Carniola, Hungary and Transylvania, &c., and he received from the Austrian Government, in 1810, a commission to examine those parts of Passau, Austria, and Bohemia, where porcelain clay is found.

Having thus attracted the notice of the Archduke John, and undertaken a journey to Styria in 1811, he was nominated Professor of Mineralogy in the Johanneum, at Gratz.

In 1818 he visited England with Count Breüner, who had been his pupil at Gratz; his conferences at Edinburgh with Jameson, whom he had known at Freyberg, made a strong impression on the Professor, in favour of what he called the "natural-history-system" of mineralogy, which he in part adopted, and first made

* On Organic Matter in Sulphureous Springs, Linn. Trans., London, vol. xvi. 1833.

† A short notice on Sulphate of Soda is published by Gimbernath in our Transactions, vol. ii., second series, p. 331.

known to British mineralogists in 1820*, and afterwards more fully explained in 1821† and 1822‡.

On the death of Werner, in 1817, he was called to the chair of Mineralogy in the Mining Academy of Freyberg; but in 1826 went to reside at Vienna, as Professor of Mineralogy, and Superintendent of the Imperial Cabinet. In 1804 he published a volume of practical importance, containing "A Detailed Account, illustrated with a Ground Plan, of the Mines and Mining Operations at Himmelsfürst, near Freyberg." In this work he describes, not only the geological relations and mineral products of these mines, but gives full details as to the methods of working them; their buildings and machinery, ventilation and drainages, preparation of the ores, receipts, expenditure, &c.

His great work on Mineralogy, or "the Natural History of the Mineral Kingdom," is best known in this country by its translation, published at Edinburgh, with considerable additions, by his pupil, Mr. William Haidinger, in 1825, 3 vols. 8vo. In the method of arrangement proposed by Mohs in this work, he founds his classification solely on external resemblances and differences, and displays a most profound knowledge of all the productions of the mineral kingdom.

This devoted pupil, friend, and successor of Werner died in Italy, 20th September, 1839, at Agordo, near Belluno, having undertaken a tour into that country for the purpose of studying the phenomena of volcanos§. He was an honorary member of the Royal and Wernerian Societies of Edinburgh.

It has been said of Mohs, and may be said of many distinguished cultivators of this department of natural science, that he was too consummate a mineralogist to be a good geologist. The sustained attention to minute details, which is indispensable to the recognition of individual minerals, gives such a habit to the mind, that it cannot easily recoil from the state of tension, which is induced by the continual study of minutiae, to that expanded condition which is essential to apprehend the magnificent generalizations of geology. For similar reasons, an extremely skilful delineator of botanic details would probably be incapable of expressing the grand and general features and effects of forest scenery, or landscape, from his habits of overstrained attention to the details of individual trees and plants that occupy the foreground of his picture.

Captain ALEXANDER GERARD, of the Bengal Native Infantry, was one of three brothers, all distinguished by their enterprising spirit, and zealous scientific researches in the Himalaya Mountains, the sons of Dr. Gilbert Gerard, who wrote the well-known "Institutes of Biblical Criticism," and grandsons of Dr. Alexander Gerard, author of works which have been translated into various European languages, and of a standard "Essay on Taste."

* Third edition of his *System of Mineralogy*.

† *Manual of Mineralogy*.

‡ *Encyclopædia Britannica*.

§ His funeral was celebrated with much ceremony expressive of public respect, and attended by a long procession of miners, each bearing in his hand a burning torch.

Having been born at the University of Old Aberdeen, in which his father was Theological Professor, he had early imbibed a thirst for knowledge and for scientific pursuits; and at the age of sixteen he entered the military service of the East India Company. Having considerable abilities as a surveyor, and being desirous of travelling, he soon got an appointment, and was sent to survey the province of Malwa, where he prosecuted his instructions under a burning sun, with great accuracy and constancy of purpose. He procured at his own expense the most costly instruments, and undertook several surveys in the Himalaya Mountains, suffering every vicissitude of heat, cold, hunger, and all the ills which could beset a traveller, with a degree of cheerfulness which was remarkable; but a residence of thirty years in India, passed chiefly in the endurance of these hardships, laid the foundation of that decay of health, which has lately brought him to a premature grave.

Captain Alexander Gerard was well known in the East as a scientific traveller, having, in company with his brother, the late Dr. James Gerard, penetrated the Himalaya Mountains through several passes before unknown to Europeans. While contributing, by his maps, to benefit geographical science, he never lost sight of what was novel and interesting in the geology, botany, and zoology of these stupendous regions, and various occasional papers have appeared from his pen, comprising valuable information on these subjects. We owe to this enterprising officer and indefatigable barometrical observer, our first knowledge of the structure of that portion of the Himalaya Mountains which forms the upper region of the Valley of the Sutlej, and is chiefly primitive. In this north-west extremity of India, on the frontier of China, he ascended to the astonishing height of 19,411 feet, on the mountain Tahigang, the summit of which he estimated at 22,000 feet above the sea.

A small collection of geological specimens made by him has been recently laid before this Society; it was formed in the district of Speetee, in Chinese Tartary, at the elevation of from 12,000 to 19,000 feet above the level of the sea, and between the latitudes $31^{\circ} 30''$ and $32^{\circ} 30''$ north, and longitude 77° and 79° east. On the confines of Chinese Tartary, at the height of 16,200 feet, he found a region of limestone containing Ammonites. The same shells occur nearly at the same height near the Niti and Manná Passes. In Thibet he observed millions of organic remains, lying at extraordinary altitudes, and forming vast and rocky cliffs. At the elevation of 17,000 feet were seen detached fragments of rocks, bearing the impression of shells, which must have been derived from still higher peaks; one cliff was a mile in perpendicular height above the nearest level.

He first appears as the companion of Herbert in his survey of the course of the Sutlej, 1819. (*Asiatic Researches*, vol. xv. p. 339.) In the same vol. p. 469, he published observations on the climate of Subathu and Kotgerh. His labours in completing a geographical survey of the valley of the Sutlej are the subject of a paper by the late Mr. H. T. Colebrooke in the *Transactions of the Asiatic*

Society of London, vol. i. p. 343. From the diary of this survey Mr. Colebrooke selected notes of Geological observations; and from specimens then collected, duplicates were sent to our Society. Upon these notes, and on Captain Gerard's letters, written during his survey in the middle valley of the Sutlej, a sketch of the Geology of the Himalaya was prepared by Mr. Colebrooke and published in the Geological Transactions of London*.

The second volume of Sir W. Lloyd's recent narrative of a journey in the Himalaya, contains an account of Captain Gerard's attempt to penetrate on the north side of the Himalaya by Bekhur, to Garoo and the lake Manasarowara, near the source of the Sutlej. These letters are interspersed with many interesting geological observations respecting the mineral productions and nature of the rocks of the country over which he travelled. He found the inclination of the strata to be usually perpendicular to the direction of the range, presenting long continuous slopes on the side towards which they dip, and terminating abruptly in rugged precipices towards the axis of the mountain chain. Near Bekhur, at the north side of the Himalaya, on the margin of the great table land of Tartary, elevated 15,786 feet above the sea, he mentions the occurrence of gravel studded with Ammonites, not far from the Hookeo Pass, which presents mural precipices of limestone.

In one excursion in the Himalaya he fell in with the late Bishop Heber, who devotes a long and eloquent passage in his journal to the expression of his praise and admiration of the scientific talent and enterprising spirit of Captain Gerard. He was an excellent Persian scholar, and acquainted with several other oriental languages.

He performed many of his surveys under a burning sun, the thermometer ranging from 100 to upwards of 112 degrees. As many of his observations were required to be taken at mid-day, the consequences were frequent suffering and illness from strokes of the sun; but he continued his labours until his health totally failed. He died at Aberdeen in December last, at the age of forty-seven, having apparently sacrificed his life to the promotion of science, stimulated in his labours by the wish to benefit mankind, without the hope of worldly remuneration.

To his late equally zealous and indefatigable brother, Dr. James Gilbert Gerard, surgeon of the Hill corps stationed at Subathu, and the companion of Captain, now Sir Alexander, Burnes, in his perilous journey through Central Asia, we owe the discovery of extensive collections of fossil shells in the Himalaya mountains, at the height of 17,000 feet. The greater part of these closely resemble shells that occur in the Oolite formation of Europe, particularly Ammonites and Belemnites; whilst a few, *e.g.* Orthoceratites and Spirifers, are similar to shells we find in rocks of our Transition Series. The Rev. R. Everest has described and figured some of these in the eighteenth volume of the Asiatic Researches.

* Vol. i., second series, p. 124.

His third brother, Captain Patrick Gerard, is remarkable as the author of a Meteorological Journal, kept in 1819-20 at Kotgerh, Subathu, and the intermediate places in the Himalaya mountains, and recording hourly observations during nearly two years*.

Feb. 26.—A paper was first read, entitled “ Further observations on the fossil trees found on the Manchester and Bolton railway ;” by John Hawkshaw, Esq., F.G.S.

Since Mr. Hawkshaw’s former communication†, another fossil tree has been found on the opposite side of the railway. It is about three feet in height, and three feet in circumference, and stands on the same thin stratum of coal as those first discovered, and perpendicularly to the surface of the bed. Mr. Hawkshaw is, therefore, strengthened in his belief, that the trees grew in the position in which they are found.

After this notice of the recent discovery, he proceeds to describe the effects produced in hot and moist climates on felled or prostrated solid dicotyledonous trees. The tropical forests with which he is acquainted from personal examination, are situated in Venezuela on the shore of the Carribean sea, and between the 8th and 10th degrees of north latitude, and the 65th and 70th of west longitude. In these forests a few months are sufficient to destroy the interior of the largest tree, little more being left than an outer shell, consisting chiefly of the bark. Mr. Hawkshaw noticed this peculiarity more frequently in dicotyledonous trees, having a proper bark, than in monocotyledonous vegetation, excluding necessarily those always hollow; and he does not remember to have seen a single instance of a palm similarly acted upon. Sometimes the portion of the dicotyledonous tree remaining on the ground, presented very much the appearance of the founder’s mould, when the pattern has been withdrawn from the sand, and before the metal has been run in; and by this kind of decay, a cavity is formed from which a fac simile of the tree might be cast. In other cases, prostrated trunks having the appearance of being solid, have yielded to the pressure of his feet, and proved to be only hollow tubes. Dangerous accidents have also occurred from temporary bridges constructed of dicotyledonous trees having given way beneath the passenger, though there was no outward indication of decay. The bark of these trees had changed but little, though nothing of the interior remained but dust, and a few remnants which crumbled beneath the slightest touch.

The low and flat tracts in which this destructive operation goes on most rapidly, are those in which, from the deep rich soil and excessive moisture, all below the tall forest trees and larger palms is occupied by canes, bamboos, and minor palms. Such tracts would be most easily submerged; and in Mr. Hawkshaw’s opinion they

* See Journal of Asiatic Society of Bengal, vol. ii. p. 615.

† L. & E. Phil. Mag. vol. xv. p. 539.

might hereafter present a seam of coal, which would afford but few distinct traces of palms and forest trees. These phænomena, he says, may explain in part, why so few distinct forms remain of the numberless forest trees, which must have formed a portion of the vegetable kingdom, at the time of the accumulation of our coal deposits.

Mr. Hawkshaw does not attempt to explain the process by which dicotyledonous trees are rendered hollow in tropical forests. He expresses doubts respecting the probable nature of the Calamites of the coal measures, and offers no explanation of the means by which they have been preserved in so great abundance. If the coal be considered as the debris of a forest, he says, it is difficult to account for not finding more trunks of trees than have been discovered in our coal basins; and he observes, it is only perhaps by allowing the original of our coal seams to have been a combination of vegetable matter, analogous to peat, that the difficulty can be solved. In this case, he is of opinion, but a few isolated trees might be expected to be found, and that the remains of vegetable forms most frequently discovered, would only be confirmative of the antiseptic qualities of their original nature, as previously advanced by Professor Lindley, and not of the number or importance of their particular genera at the time of their deposit.

In conclusion, Mr. Hawkshaw says, that whatever opinion may be drawn from what is conjectural in his paper, it will be obvious, that though fossil remains may be found filled with a mechanical deposit, and containing traces of other vegetables, yet that this condition does not prove, that the plants were originally hollow, nor even render it the most likely hypothesis, as they may have been hard wood-trees, the centre of which had been removed by natural processes.

LXXVI. *Intelligence and Miscellaneous Articles.*

BLUE OXIDE OF TITANIUM.

M. KERSTEN has shown that oxide of titanium may be prepared containing less oxygen than titanous acid, and that this blue substance, which has hitherto been prepared only in the moist way, may be obtained in the dry way, and in several modes:

1st. By passing the vapour of zinc over titanous acid heated to whiteness; the acid then assumes a dirty blue colour, but loses it, and becomes again white when exposed to oxygen at a high temperature.

2ndly. By putting into a porcelain crucible, metallic zinc, or oxide of zinc mixed with charcoal, and then covering the whole with titanous acid or a titaniferous earthy silicate, and heating the crucible for several hours to whiteness. Fused masses of a lavender blue colour are thus obtained; they are opaque, and contain blue oxide of titanium and oxide of zinc.

3rdly. By fusing at a white heat, and out of contact of the air several titaniferous compounds, as, for example, the titaniferous silicates of lime with iron. These silicates, which are at first colourless, become more or less blue.

4thly. By treating the same titaniferous silicates in the same manner with metallic tin. A slight addition of charcoal in powder appears to favour the formation of blue glass.

5thly. M. Kersten found that titanitic acid might, under certain circumstances, be reduced, by means of hydrogen, to the state of blue oxide of a very fine colour. If into biphosphate of soda, kept in fusion in a porcelain crucible, fine titanitic acid be projected, it dissolves readily at a low temperature, and a white, transparent saline mass is formed. If this mass be put into a bulb of difficultly fusible glass, and hydrogen gas which has been dried by chloride of calcium be passed over it, the saline mass soon becomes of a fine lavender blue at the surface; if the residue be afterwards dissolved in water, the phosphate dissolves, and oxide of titanium of a fine blue colour remains; this oxide was left in contact with water during two months, without producing any diminution of its fine colour; but when heated to redness in contact with the air, it loses its colour, and is converted into titanitic acid. This blue compound is not acted upon by hydrochloric acid at common temperatures; but when heated with this acid it loses its colour, and is converted into titanitic acid. Zinc, tin, and iron immersed into this solution, reproduce the blue oxide.

6thly. If pure zinc be placed at the bottom of a porcelain crucible, and then above it a mixture of titanitic acid and biphosphate of soda, or what is still better, if these substances fused together, be strongly heated in a crucible for several hours, a blue saline mass is obtained, sometimes of a violet tint, which dissolves in water, leaving a residue, the blue substance, mixed with a little oxide of zinc, which is not dissolved by the alkaline phosphate.

7thly. If instead of zinc, tin or iron be employed, the results are similar; but by the two last methods the blue oxide obtained is, however, not of so fine a colour as when the titanitic acid is reduced by hydrogen.

CHELIDONIA AND PIRROPINA.

M. Potex has lately discovered two alkalis in the root of the *Chelidonium majus*; to the former he has given the name of *chelidonia*, and to the latter *pirropina*. To prepare these, the dried root of the *Chelidonium* is to be powdered and twice boiled in spirit; these infusions being distilled, they are poured into a cucurbit, and an equal quantity of distilled water being added, the alcohol is to be separated by another distillation. The residue is to be poured into a capsule; by perfect cooling a soft resin separates; the filtered liquor is to be precipitated by carbonate of soda, which throws down the two alkalis. Filtration is to be performed again, and the pre-

cupitate, after washing with cold water, is to be dissolved in boiling alcohol; the solution is to be filtered and allowed to cool. In this operation the greater part of the chelidonia crystallizes, and by evaporation more is obtained. The crystals are to be washed with alcohol to separate extractive. The mother-water and the washings are to be evaporated by a gentle heat, and then the pirropina crystallizes with a little chelidonia; these crystals are in yellow laminæ, and a portion is deposited on the sides of the vessel in blackish discs.

The properties of chelidonia are, that it crystallizes partly in transparent tables and partly in cubes, and varieties. In the state of crystals it dissolves with difficulty in hot alcohol and in æther; it crystallizes on their cooling. The concentrated acids, even when heated, act slowly on this substance; nitric acid gives it a yellow colour, and sulphuric acid blackens it. The diluted acids form colourless salts with it, which readily crystallize and have an astringent and very bitter taste. Chelidonia dissolves readily in the fat and volatile oils when heated; the solutions have a bitter taste.

The alcoholic solution has an alkaline reaction. If tincture of galls or subacetate of lead be added to a solution of acetate of chelidonia, abundant white precipitates are obtained. Tincture of iodine forms a kermes-coloured precipitate, chromate of potash a deep yellow, chloride of gold a dirty reddish yellow, and the alkalis give white precipitates.

The properties of pirropina are, that it forms stellated crystals, which are aggregated prisms; they are colourless and transparent, and on cooling they lose their transparency and become slightly brown. The acids, when cold, act upon it but slightly, but when heated they dissolve it, and become of a golden yellow or reddish colour; a powerful acid imparts a fine flame-red colour to the crystals. Its compounds are generally but little soluble in cold water, and have a slightly bitter taste, but are acrid and penetrating. Pirropina dissolves with difficulty in cold æther and alcohol, but readily when they are boiling. The fat and volatile oils dissolve it when hot. It fuses when heated, burns and yields ammoniacal vapours. Tincture of iodine precipitates acetate of pirropina of a crimson colour; chloride of gold yellowish brown, chromate of potash deep brown; tartar emetic, chloride of iron, proto-nitrate of mercury, bichloride of mercury, and nitrate of silver, all give yellowish white precipitates.

The alkalis form a white precipitate; tincture of galls and subacetate of lead produce no effect; the alcoholic solution has no alkaline reaction.—*Journal de Chim. Médicale*, Août, 1840.

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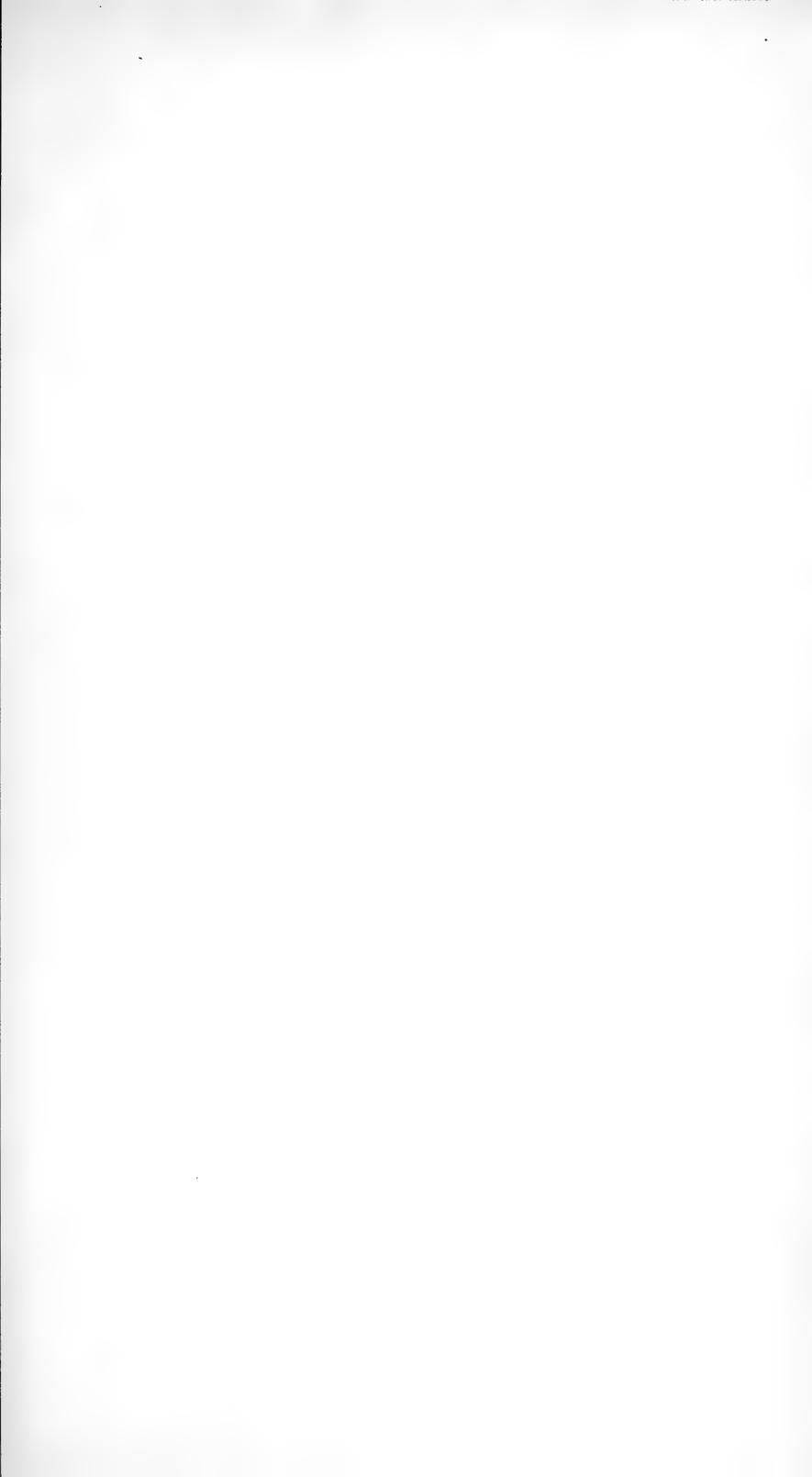
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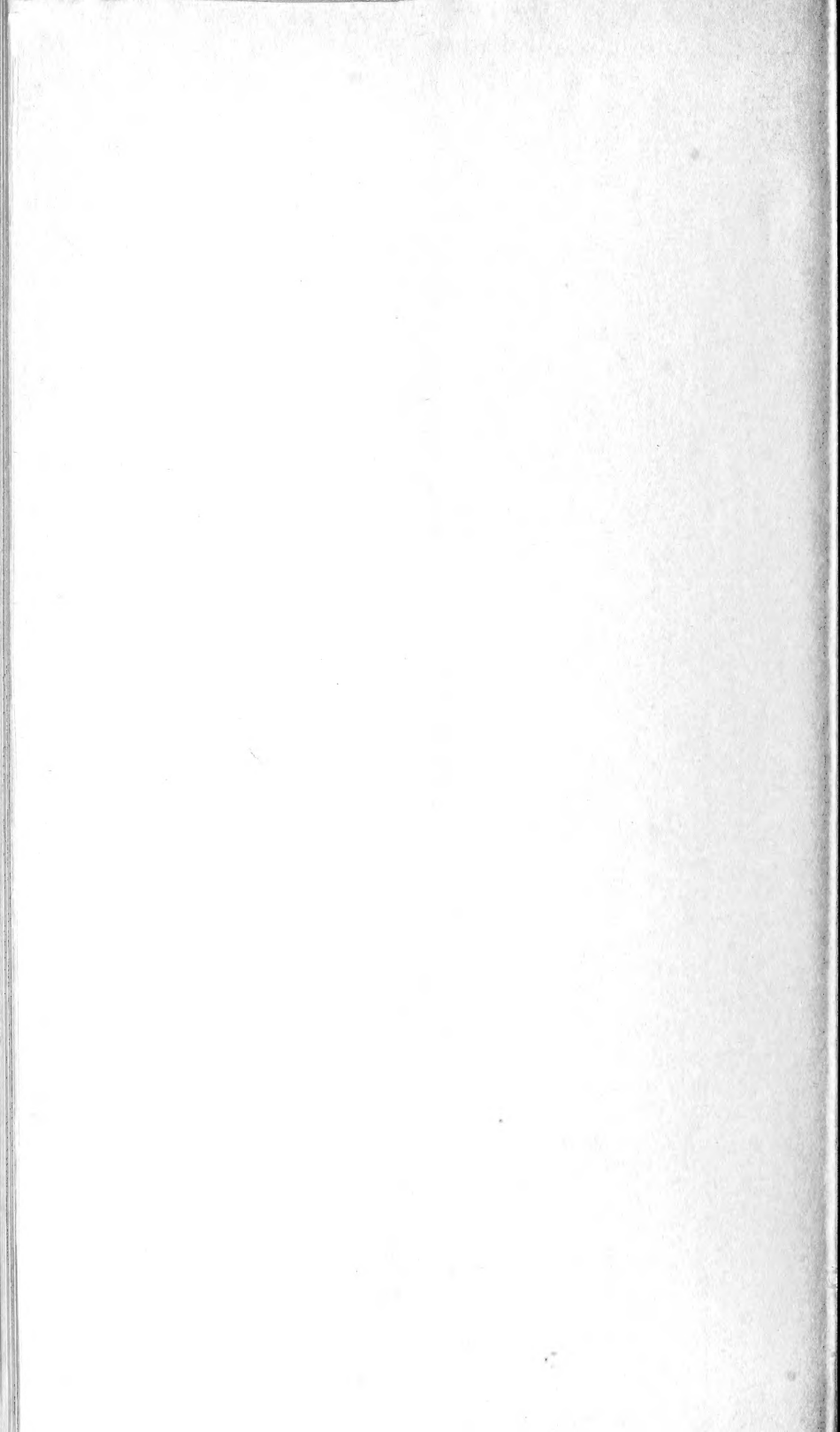
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